August 30, 2025

Chris Wright, Secretary
U.S. Department of Energy
1000 Independence Ave., SW
Washington, DC 20585

Re: A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate, Docket ID No. DOE-HQ-2025-0207

Dear Secretary Wright:

On behalf of the more than 85 experts who contributed to the *Climate Experts' Review of the DOE Climate Working Group Report*, we are writing to submit this compendium of public comments on the Climate Working Group (CWG) Report entitled *A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate*, under docket number DOE-HQ-2035-0207.

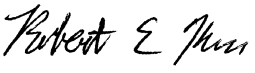
This compendium begins with a Summary for Policy Makers that highlights a selection of key points from across the constituent Comments. It then contains 48 distinct Comments, each of which makes a variety of points regarding different elements of the CWG report and the report as a whole. Authorship of each Comment is separately noted. It concludes with biographical sketches providing the authors' credentials.

We hope the Summary for Policy Makers and the 48 Comments will be given full consideration and that, as with public comments on National Climate Assessments, there will be a substantive response to each and every element of these comments.

As explained at length in these comments, the CWG report currently fails to adequately represent the scientific understanding of climate change. DOE must subject the CWG report to unbiased, robust, and transparent peer review under the Information Quality Act and other federal requirements. Accordingly, it will require very substantial revision before it can be relied upon by any federal agency or other entity.

Sincerely,

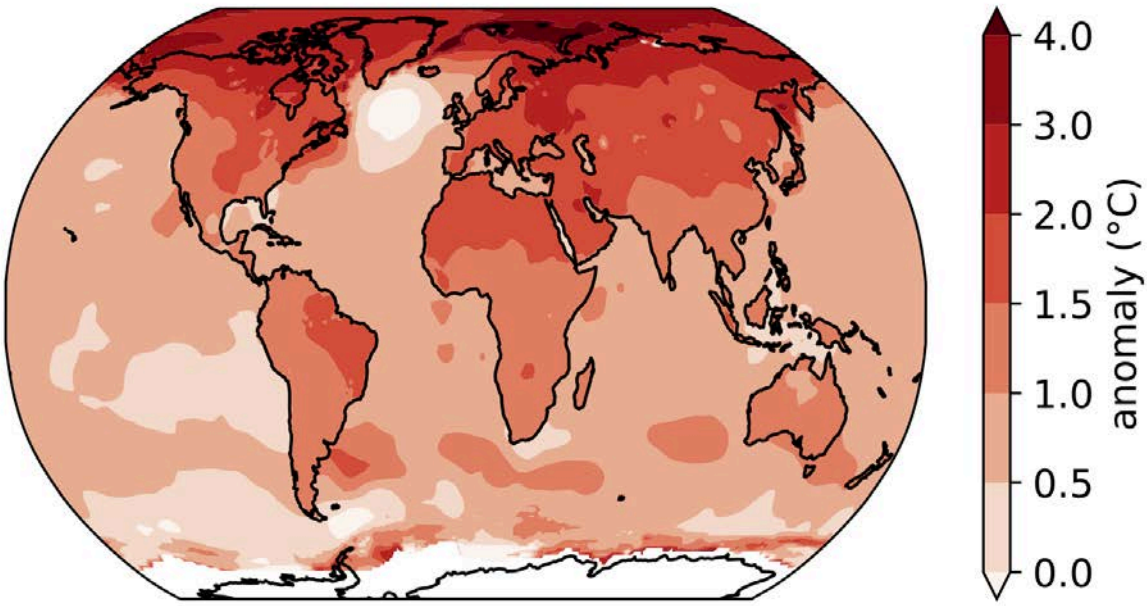
Andrew E. Dessler, Ph.D.
Co-Editor, *Climate Experts' Review of the DOE Climate Working Group Report*



Robert E. Kopp, Ph.D.
Co-Editor, *Climate Experts' Review of the DOE Climate Working Group Report*

Climate Experts' Review of the DOE Climate Working Group Report

Comment submitted to the U.S. Department of Energy, docket number DOE-HQ-2025-0207, in response to their report "A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate"



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Climate Experts' Review of the DOE Climate Working Group Report

Summary for Policy Makers

Andrew E. Dessler, Texas A&M University
Robert E. Kopp, Rutgers University

The basic science of Earth's climate has been well established through two centuries of research. Over the 16 years since the Environmental Protection Agency's 2009 Endangerment Finding, the evidence for human-caused climate change and the dangers it poses to public health and welfare has continued to grow stronger. The report authored by the five members of the Department of Energy (DOE) Climate Working Group (CWG) and released by DOE on July 29, 2025, fails to adequately represent this reality.

This document is an expert review of the DOE CWG report. It represents the collective, self-organized efforts of more than 85 climate experts over the report's short, month-long comment period. As our review details, the CWG report exhibits pervasive problems with misrepresentation and selective citation of the scientific literature, cherry-picking of data, and faulty or absent statistics.

The DOE CWG report does not meet standards of quality, utility, objectivity and integrity appropriate for use in supporting policy making. In assessing the science of climate change, the U.S. government should follow standards appropriate for a Highly Influential Scientific Assessment, including an unbiased, robust, and transparent peer review process. Rigorously reviewed assessments, like the Intergovernmental Panel on Climate Change and National Climate Assessment reports, provide much more accurate representations of the scientific understanding of climate change.

The basic science of Earth's climate has been well-established through two centuries of research. Over the 16 years since the Environmental Protection Agency (EPA)'s 2009 Endangerment Finding, the evidence for human-caused climate change and the dangers it poses to public health and welfare has continued to grow stronger.

Recent assessments, such as the Intergovernmental Panel on Climate Change (IPCC)'s 2021 Sixth Assessment Report (AR6), confirm that the evidence shows that human influence is warming the atmosphere, ocean, and land in a way that is unprecedented for many centuries to millennia. The evidence that human-caused climate change is changing heat waves, heavy

rainfall events, droughts, hurricanes, and wildfires has grown. Across the coastal U.S., outside of parts of Alaska, rising sea levels are incontrovertibly causing an increase in coastal flooding.

The frequency, intensity, and duration of heat waves is increasing globally as a result of long-term, human-caused warming driven by greenhouse gas emissions.[1] **[Comment (Cmt.) 20, 37]** The magnitude and intensity of extreme precipitation has very likely increased since the 1950s in North America as a whole, and particularly in eastern North America.[2] **[Cmt. 21]** Climate change has intensified hydrological, ecological, and agricultural drought in the western U.S. and other regions around the world over the past few decades.[3] **[Cmt. 24]**

Although no trends in the number of hurricanes making landfall in the U.S. have been detected (and none were expected), hurricanes are becoming more hazardous. They are reaching higher intensity and intensifying more quickly. Higher sea levels are making their coastal flooding more extensive and damaging, and atmospheric warming is leading them to produce more rainfall, which is increasing their inland impacts.[4] **[Cmt. 18]**

Climate change has led to increased fire weather, longer fire seasons, and drier fuels. As a consequence, the likelihood and intensity of the most extreme wildfires has increased in North America, with major effects in the western U.S. These extreme fires have generated substantial ecological, economic, health, and livelihood impacts across the U.S. The consequences extend far beyond fire-prone regions due to smoke transport across continental distances, exposing populations nationwide to serious health risks.[5] **[Cmts. 25, 26]**

Sea-level rise is increasing risk to coastal communities. The rate of global-mean sea-level rise has more than doubled over the last 30 years, from about 0.08 inches/year in 1992 to 0.18 inches/year in 2024, and statistical analysis also reveals sea-level acceleration at many U.S. tide gauges and in contiguous U.S. tide gauges in aggregate. Along parts of the U.S. Atlantic and Gulf coasts, the number of days of coastal flooding per year have increased more than ten-fold since the 1970s as a result of sea-level rise, and relative sea-level rise is projected to increase high-tide flooding by 5-10x on average across the country's coast by 2050.[6] **[Cmt. 27]**

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[1] The DOE CWG Report claims otherwise based on a fallacious interpretation of Dust Bowl conditions in the U.S. in the 1930s; see discussion below.
[2] The DOE CWG Draft Report claims otherwise based on limited sampling of the available data and precipitation extreme metrics.
[3] The DOE CWG Report focuses exclusively on meteorological droughts, which are evaluated based on low rainfall alone. Other definitions of drought are more relevant to impacts on public welfare. Agricultural and ecological drought are defined as water insufficient to meet agricultural and ecological needs, and factor into account phenomena like soil moisture and water balance (evaporation minus precipitation). Hydrological droughts are assessed based on low surface water flow and changes in runoff.
[4] The DOE CWG Report focuses on the (unsurprising) absence of a trend in landfalling hurricane frequency, while ignoring the evidence that hurricanes are becoming more hazardous.
[5] Illustrating the single-cause fallacy, the DOE CWG ignores the evidence for changes in fire weather and focuses exclusively on the role of fire management practices.
[6] The DOE CWG Report claims there is "no obvious acceleration in sea-level rise". It does not provide any statistical analysis to support this claim, nor does it discuss the extensive scientific literature demonstrating acceleration. The CWG Report does not discuss the present or future impacts of sea-level rise.

Economists have concluded that these and other effects of climate change will likely have significant negative impacts on the U.S. and global economy. Every additional ton of greenhouse gas emitted into the atmosphere increases global average temperature, and every additional amount of global warming escalates the danger to public welfare.[7] **[Cmt. 43]**

The report authored by the five members of the Department of Energy (DOE) Climate Working Group (CWG) and released by DOE on July 29, 2025, fails to adequately represent the current scientific understanding of climate change.

In contrast to the requirements for a committee established under the Federal Advisory Committee Act (FACA), no attempt appears to have been made to balance the points of view represented on the CWG; rather, this group appears to have been personally recruited by the Secretary of Energy to advance a particular viewpoint favored by DOE leadership.[8] The CWG members have, to their credit, previously published some of the ideas reflected in the CWG report in the peer-reviewed literature. The scientific community has previously examined them and delivered a collective, resounding rejection.[9]

Beyond the unbalanced membership of the CWG, the process that led to the CWG report does not include other guardrails to minimize bias in the report. For example, in contrast to the requirements for a FACA committee, the CWG's deliberations have been conducted in secret and without public record or notice. This also stands in contrast to the aims proclaimed by Executive Order 14303 ("Restoring Gold Standard Science"), which calls for science to be conducted in a manner that is transparent, without conflicts of interest, and subject to unbiased peer review.

It is also worth noting that an assessment of this type would normally have hundreds of authors, and would be written over the course of multiple years, with multiple rounds of internal and external review. A large author team allows each section to be written by experts on that exact topic. The DOE CWG report, due to its very small writing team, covers areas in which the authors are not experts and has led to many errors in the report caused by a lack of familiarity with the science.[10]

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[7] As discussed below, the DOE CWG Report focuses on the small number of studies suggesting climate change is less damaging than most analyses suggest is most likely, while ignoring the larger number of studies suggesting climate change is more damaging than central estimates.

[8] https://www.cnn.com/2025/08/07/climate/wright-national-climate-assessments-updating

[9] The Report relies unusually heavily on citations to the authors' own work. Overall, nearly 11% of all citations in the report are to the work of one of the report authors. In chapters 3 and 5, more than a quarter of citations are self-citations. This compares to an average of 2.4% and a chapter maximum of 5.1% in the IPCC AR6 Working Group 1 report. **[Cmt. 48]**

[10] For example, the authors exhibit basic confusion about the concepts of Climate Impact Drivers and time of emergence **[Cmt. 36]**. They do not understand how the climate component of integrated assessment models are calibrated **[Cmt. 43]**. They do not correctly define relative sea-level change, and highlight as a major conclusion the important role of vertical land motion in relative sea-level change – something that has been understood since the nineteenth century **[Cmt. 27]**.

It is important to note the severe time constraints imposed on reviewing this document. The 30-day review period was inadequate for conducting a thorough and systematic analysis of this report.[11] Given that, we have undoubtedly failed to identify numerous additional errors, misrepresentations, and critical omissions throughout the text, and we have made errors of our own. The deficiencies highlighted in our comments should therefore be viewed as the tip of the iceberg — the document's true problems almost certainly extend far beyond what we were able to catalog in the limited time available.

This document is an expert review of the CWG report. It represents the collective, self-organized effort of more than 85 climate experts over the short, month-long comment period invited by Federal Register notice 90 FR 36150.

We are submitting our expert review in response to this Federal Register notice, but do so in full recognition that the notice provides no details on the review process.

The DOE CWG Report clearly meets the definition of a Highly Influential Scientific Assessment (HISA) under the Office of Management and Budget (OMB)'s Final Information Quality Bulletin for Peer Review (70 FR 2664). As such, OMB and EPA guidance require an unbiased peer review comparable in rigor to that conducted by the National Academies, as well as opportunities for public participation in report review and full documentation of responses to peer review comments. **[Cmt. 47]**

The obscure nature of the CWG review process stands in contrast to the process employed by National Climate Assessments (NCAs), which comply with the requirements for HISAs. The NCA reports undergo multiple stages of expert and public review, including peer review by the National Academies of Sciences, Engineering, and Medicine. The NCA's review editors — a group of experts independent of the authors — ensure that the report authors seriously consider and provide a public, written response to every substantive public and National Academies comment.[12] We do not know whether to expect from the CWG a similarly transparent, thorough, and independently overseen response to comments like ours.

As detailed in the over 400 pages of our expert review, the DOE CWG Report exhibits pervasive problems with misrepresentation and selective citation of the scientific literature, cherry-picking of data, and faulty or absent statistics.

Below, we discuss some high-level examples.[13]

[11] By comparison, the Third Order Draft of the Fifth NCA allowed for a 12-week comment period.
[12] While GlobalChange.gov, the Internet home of the US Global Change Research Program for more than a quarter-century, was shut down on June 30, 2025, the annotated responses to comments on the 2023 Fifth National Climate Assessment can be accessed through the Internet Archive at https://web.archive.org/web/20250630050603/https://nca2023.globalchange.gov/downloads/.
[13] For an overview of rhetorical strategies and reasoning fallacies that often appear in climate misinformation, see Cook, J. (2020). Deconstructing climate science denial. In D. C. Holmes & L. M. Richardson (Eds.), *Edward Elgar Research Handbook on Communicating Climate Change* (pp. 62–78). Edward Elgar Publishing. https://doi.org/10.4337/9781789900408.00014

Climate projections are driven by scenarios of future emissions of greenhouse gases and other climate forcers. The DOE CWG Report broadly claims that "scenarios widely-used in the impacts literature have overstated observed and likely future emission trends" (p. ix). This ambiguous language misrepresents the fact that climate scenarios are deliberately constructed to span a broad range of possible futures, from very low emissions to very high emissions. Current emissions of carbon dioxide from fossil fuels and land use are tracking near the middle of the scenario range used in AR6. Under AR6's Intermediate emissions scenario (SSP2-4.5), which assumes declining global CO_2 emissions in the second half of the century, late-century warming will very likely be 2.1-3.5°C above late nineteenth-century levels (about 1.5-2.9°C warmer than the 1990 baseline used in the EPA's Endangerment Finding). **[Cmt. 4, 6]**

The DOE CWG Report argues that climate models do not accurately describe recent climate trends. Toward that end, the DOE CWG cherry-picks evidence that support a low equilibrium climate sensitivity (ECS)[14] while ignoring or downplaying an overwhelming body of contradictory evidence that collectively led the IPCC to conclude that ECS is likely between 2.5°C and 4.0°C for a doubling of carbon dioxide concentrations. It also focuses on the highly uncertain, multi-century metric of ECS over less uncertain, more policy-relevant metrics, like those characterizing the nearer-term response of the climate to carbon dioxide emissions. **[Cmt. 9, 10]**

Indeed, in a separate part of its report, the DOE CWG Report accurately highlights the recent rapid increase in global mean surface temperature, with temperature in 2023 surpassing the previous record by nearly 0.2°C, and elevated temperatures persisting into 2025. As the DOE CWG Report notes, this is a topic of active scientific research, and might be tied to a decrease in the planet's albedo (reflectivity). One possible cause of this decrease is a potential strong positive cloud feedback; if true, this would argue for a higher ECS. This discussion is, however, ignored in the report's argument that climate models are too sensitive to greenhouse gas-induced warming. **[Cmt. 9, 35]**

The DOE CWG Report highlights the Dust Bowl period of the 1930s as a time of unusually hot temperatures in the United States, a period that in parts of the contiguous United States still dominates high temperature records. This is another example of cherry picking, as well as an example of the single cause fallacy (specifically, the idea that, because climate has varied naturally in the past, current changes must also be natural). However, over such a small area, natural variability plays a very large but temporary role. The Dust Bowl was indeed an exceptional event, and the decadal climate conditions that drove the Dust Bowl are not occurring today. Thus, the comparison between the 1930s and today does not tell us about the impact of anthropogenic climate change on today's extreme temperatures. The fact that high-temperature records are being reliably broken more frequently than low-temperature records – both globally and in the U.S. – is a clear indicator that humans are making extreme high temperatures more extreme. **[Cmt. 19, 20]**

[14] ECS measures the long-term (millennial-scale) response of global mean temperature to a fixed forcing, conventionally taken as an instantaneous doubling of carbon dioxide concentrations from their preindustrial levels

To argue that climate models overestimate warming, the DOE CWG Report, in another example of cherry-picking, highlights the U.S. Corn Belt, an area which makes up less than 1% of the area of the planet. The Corn Belt is the most agriculturally productive region on the planet – an area where land-use changes and natural variability over the last few decades have largely offset greenhouse gas warming. To the extent that models have problems in this very small region, it is connected to how well the models are handling the specific land-use changes occurring only in this region, not how well they represent global warming. In fact, in areas across the Northern Hemisphere, climate models are equally likely to overestimate or underestimate warming – exactly as expected for models that get the warming right. **[Cmt. 17]**

While in some cases, climate change can yield local benefits, on net its effects are harmful to welfare and economic output in the United States. **[Cmt. 38-43, 45]**

Although increasing carbon dioxide concentrations in isolation can increase crop yields, the DOE CWG report's focus on carbon fertilization is an oversimplification. In the real world, crops must simultaneously contend with the climatic effects of increasing greenhouse gases, including hotter temperatures and enhanced evaporation. The evidence suggests that CO_2-induced climate change, including rising temperatures, shifting weather patterns, and altered precipitation, will outweigh the benefits of CO_2 fertilization, leading to net yield declines across major agricultural regions in the U.S. and globally compared to what would happen without rising CO_2 concentrations. **[Cmt. 38]**

Although the DOE CWG Report notes that cold-related deaths in the United States are higher than heat-related deaths, their discussion overlooks the complexity of how climate change will reshape mortality patterns. While global warming may initially reduce deaths in some regions and increase them in others, heat-related mortality will increasingly dominate as temperatures continue to rise. The report also makes unrealistic assumptions about adaptation through air conditioning, failing to account for economic barriers that prevent access. Even in wealthy countries like the U.S., the poorest communities struggle to afford air conditioning, and this challenge is far more severe in developing nations. The DOE CWG report also ignored the fact that climate change has health impacts other than temperature-related mortality. These include a variety of pathways other than temperature, such as vector-borne diseases (like malaria and Lyme disease), food security and malnutrition, and air quality. **[Cmt. 41, 42]**

Human well-being is intertwined with the stability of natural systems: from safe food and clean water to disease regulation, mental health, and cultural identity. The DOE CWG Report omits discussion of the disruptive effects of climate change on wildlife and biodiversity, which are altering predator-prey relationships, increasing disease risks, and destabilizing ecosystems that underpin human well-being. The resulting economic impacts include reduced pollination and fisheries yields, damage to aquaculture, declines in ecotourism, costs from invasive species and harmful algal blooms, and cultural losses tied to species extinctions. By neglecting biodiversity impacts, the DOE CWG Report misses a key pathway by which greenhouse gas emissions endanger human health and prosperity. **[Cmt. 45]**

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Adaptation is essential to manage climate risk, but it cannot eliminate it. For example, adaptation can reduce, but not eliminate, damages associated with future sea-level rise. Yet the United States is not optimally adapted even to current coastal hazards. Economically efficient adaptation will require substantial investments by individuals, industries, and all levels of government well in advance, and failure to make such investments will decrease economic stability. Furthermore, many low-lying regions will reach adaptation limits over the course of this century. However, the discussion of adaptation in the DOE CWG Report is very limited, and does not consider the costs of and barriers to either private or public adaptation, about which there is a significant literature. **[Cmt. 27]**

The climate can only be stabilized through the cumulative effect of many mitigation actions, and each marginal effort brings benefits for human welfare. In its discussion of the economic costs of climate change – and thus of the economic benefits of reducing greenhouse gas emissions – the DOE CWG Report looks at uncertainty about climate change and its impacts through rose-tinted glasses. It focuses not even on the most common estimates of possible outcomes, but on a cherry-picked set of outcomes where everything turns out better than most analysts expect, let alone fear. A long history of work on risk management points to the importance of considering the full distribution of outcomes, including low- or unknown-likelihood outcomes associated with high-impact risks. The DOE CWG's general approach would also argue against the purchase of insurance, the maintenance of a standing army in peacetime, and many other regularly accepted precautionary measures. **[Cmt. 43, 44]**

In short, the DOE CWG Report does not meet standards of quality, utility, objectivity and integrity appropriate for use in supporting policy making. In assessing the science of climate change, the U.S. government should follow standards appropriate for a Highly Influential Scientific Assessment, including an unbiased, robust, and transparent peer review process. Rigorously reviewed assessments, like the Intergovernmental Panel on Climate Change and National Climate Assessment reports, provide much more accurate representations of the scientific understanding of climate change.

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About the writing team

This report arose organically when Dr. Andrew Dessler asked on Bluesky whether anyone was coordinating a response to the DOE CWG Draft Report. [15]Following his inquiry, multiple people encouraged him to take on the coordination role, which he ultimately agreed to do. He then put out a call for volunteers on social media. Many signed up and, as word spread, more scientists contacted him and volunteered to write a section. As the workload expanded, Dr. Robert Kopp volunteered to be co-editor of the effort.

In the end, more than 85 scientists contributed. Authors are mostly from the U.S., but with important contributions from Europe, Asia, Australia, and Canada. The age distribution was evenly split between early career, mid career, and senior. About 80% of the authors work at universities, with the rest at a variety of other institutions.

This report is the result.

Each section was authored by a particular writing team and responsibility for the contents of that section rests solely with them. Affiliations listed for any authors are included for identification purposes only. The opinions, findings, conclusions, and recommendations expressed are those of the authors and not their institutions. Some authors wished to remain anonymous, and they are not listed.

We would also like to acknowledge the editing team that took the disparate sections and, despite the extremely short time frame, put this document together: Lauren Cagle, Chris Colose, Jillian Gregg, Genevieve Guenther, Marc Jones, William Ripple, and Emily Williams.

[15] https://bsky.app/profile/andrewdessler.com/post/3lv4wamrpes2e

Response to Chapter 2, Section 2.1: CO2 as a contributor to global greening

David J Beerling, Ph.D.; Benjamin Brown-Steiner, Ph.D.; Anna B. Harper; PhD.; Rosie A. Fisher, Ph.D.; Rebecca B. Neumann, Ph.D.; Ben Poulter, Ph.D.; Scott R. Saleska, PhD.; Abigail L.S. Swann, PhD.; Emily Williams, Ph.D.

Overview of Comments on this Section

Pages 3-6

Comment Type: Technical

Section 2.1 focuses on the role of CO_2 in causing "greening," or the enhancement of photosynthesis. The section overlooks the fact that it is the net carbon balance of terrestrial ecosystems that matter for the climate system, not just the rate of photosynthesis. The effect of CO_2-driven changes to climate, including increased temperature and changes in water stress, on terrestrial net carbon balance are not discussed in any sections of the report, leading to the false impression that increased CO_2 levels *only* result in an improvement in plant productivity. The net uptake of carbon by terrestrial ecosystems is expected to saturate as climatic changes continue. The uptake will also possibly reverse. Section 2.1 of the DOE Report implies that the IPCC is masking or hiding the well-established phenomenon of CO_2 fertilization of plant growth, when, in reality, it informs the IPCC assessments as a foundational principle of the models used to simulate the terrestrial carbon cycle. CO_2 fertilization, moreover, remains only one of many competing factors that drive plant productivity, ecosystem health and net terrestrial carbon balance.

ACTION: Report authors need to include discussion of all the CO_2-driven climate changes that affect ecosystems – beyond the 'greening' effect – and include discussion of all the components of the terrestrial carbon budget, not just plant photosynthesis.

Below we provide specific comments to statements made throughout Section 2.1

Chapter Summary

Page 3

Comment Type: Technical

DOE CWG STATEMENT: "*The growing CO_2 concentration in the atmosphere has the important positive effect of promoting plant growth by enhancing photosynthesis and improving water use efficiency. That is evident in the "global greening" phenomenon discussed below, as well as in the improving agricultural yields discussed in Chapter 10. Here we focus just on CO_2 fertilization; research on combined effects due to temperature and precipitation changes are discussed in Chapter 10."*

COMMENT: The impact of elevated CO_2 on agriculture is discussed in Chapter 9 of the DOE CWG Report, not Chapter 10. This is a typo in the report that needs to be fixed. We will refer to the correct chapter number (i.e., Chapter 9) in our comment. As highlighted in the detailed responses to Chapter 9, it is a misrepresentation of the science to state that growing CO_2 concentrations will improve agricultural yields. In addition, Chapter 9 includes minimal discussion of "research on combined effects due to temperature and precipitation changes," as highlighted in the comments to Chapter 9. The report authors need to add discussion of these combined effects to Chapter 9 and include the studies cited in the comments that were made to Chapter 9.

The section that focuses on "CO_2 as a contributor to global greening" is inaccurate, because it looks solely at the effects of CO_2 (via CO_2 fertilization) without looking at the other effects of CO_2 (on increased temperature, etc.; see Grossiord et al. 2020). Ignoring the combined effects that rising CO_2 levels will have on the Earth system (e.g., warmer temperatures and altered precipitation patterns) means the report does not provide a robust assessment of the true impact that rising CO_2 levels will have on plant growth and ecosystems.

In addition, not all plants respond similarly to enhanced CO_2. The concept of global greening does not account for changes to species composition in ecosystems, which could have important implications for biodiversity and ecosystem health (Bond & Midgley, 2012; O. Phillips et al., 2002). For example, shrub growth into savannas is linked to higher atmospheric CO_2 levels (Manea & Leishman, 2019; Nackley et al., 2018). Uniformly declaring the effects of enhanced CO_2 as 'positive' is misleading.

ACTION: The introduction to Section 2.1.1. should mention other effects of higher CO_2 levels in the atmosphere (rising temperatures, altered precipitation patterns, increased aridity, greater risk for fires, pests, and disease). A more thorough discussion of the many impacts of these factors on plant growth, agriculture, and biodiversity should be included within the report. Critical references that should be included in this report can be found in our comment below to section 2.1.1, last paragraph page 5. The authors should caveat the term 'positive' to more accurately reflect the balance of changes that occur with higher levels of CO_2. Remove "as well as in the improving agricultural yields discussed in Chapter 10."

2.1.1. Measurement of global greening

Page 3-4

Comment Type: Technical

SUMMARY OF COMMENTS ON THIS SECTION: Section 2.1.1 deals with global greening, but we found statements that mask the complexities of patterns and trends noted by greening studies. Global greening has occurred, but the drivers underlying the trend are complex and can be affected by factors other than CO_2. We suggest multiple changes so the report can more accurately describe the varying effects of CO_2 on global patterns detailed below.

Page 3

Comment Type: Technical

DOE CWG STATEMENT (first paragraph of section 2.1.1, page 3): "*Greening" refers to an increase in the fraction of the Earth's surface covered by plants. It can be quantified by the "Leaf Area Index" (LAI) measured by satellite."*

COMMENT: The definition of 'greening' used in the DOE report is overly simplistic, and it is incorrect to say that it is quantified by changes to LAI only. Greening refers to changes in leaf area index (LAI) or the fraction of photosynthetically absorbed radiation (FPAR) over time within a defined area (Piao et al., 2020). The DOE report uses a definition that implies 'greening' is an expansion of vegetation into areas that were previously non-vegetated. This implication is false, which is important because the DOE report thus incorrectly interprets the literature on 'greening' throughout this section.

ACTION: The report authors need to correct the definition of greening to include changes in vegetation indices based on FPAR and the subsequent interpretation of the implications of greening.

Page 3

Comment Type: Technical

DOE CWG STATEMENT (first paragraph of section 2.1.1, page 3): "*Zhu et al. (2016) was one of the first studies to report that global greening was detectable using satellite sensors. From 1982 to 2011 they detected greening over 25-50 percent of the Earth versus "browning" over only four percent and attributed 70 percent of the greening to rising* CO_2 *levels (see Figure 2.1)."*

COMMENT: The Zhu et al. (2016) paper is among many prominent papers cited by the IPCC that document the long literature on global greening and browning (c.f. Myneni et al. 1997; Nemani et al 2003; Zhao and Running 2010). The more recent literature highlights slowdowns in greening attributed to the changing climate (Yuan et al 2019) and provides a richer accounting of the effects on 'greening' biome differences and land use.

Cortés et al (2021) make improvements on the statistical approach for identifying regions of greening, and find a much reduced area of greening that covers only 15.3% of the land surface.

Yuan et al. (2019) find that the 'global greening' trend reversed across most of the world and, in the US, was concurrent with a sharp increase in VPD observed starting in the late 1990s. In other words, they find that while the CO_2 fertilization effect dominated from 80s-90s, after the late 1990s, VPD partially (but not fully) counteracts it. Winkler et al. (2021) find a slowing down of greening and strengthening of browning trends, particularly in the last 2 decades, and they also find that most models substantially underestimate the emerging vegetation browning, especially in the tropical rainforests. Piao et al. (2020) report that most of the greening is taking place in boreal ecosystems, with the tropics greening less or browning, due to negative effects of climate change. Along these lines, Figure 2.1 is reproduced from Zhu et al. (2016) but fails to acknowledge that stippling on the map indicates statistically significant trends, and because only some regions are stippled this shows that trends in many regions are not robust. Furthermore, the location of greening has important implications for how any increases in photosynthesis translate into climate- relevant ecosystem properties such as net land carbon sinks, as well as for the impact on climate of changes in leaf area, given that a higher expanded leaf area index is typically associated with warming in boreal regions (Y. Li et al., 2023; W. Zhang et al., 2018).

ACTION: The report authors need to update the estimated regions of greening and browning to be consistent with more recent publications (i.e. Winkler et al., 2021, Cortés et al., 2021, Piao et al., 2020) and to discuss which regions are experiencing greening or browning with statistical significance. Figure 2.1 caption needs to add that only stippled points were found to have statistically significant trends. Further the report needs to add discussion of the implications of greening trends in specific regions that may be the result of anthropogenic driven land cover change, i.e. agricultural land management or afforestation (Piao et al. 2020).

Page 3

Comment Type: Technical

DOE CWG STATEMENT (second paragraph of section 2.1.1, page 3): "*Zeng et al. (2017) confirmed the pattern of greening, noting that over thirty years it had added 8 percent to global leaf area and that greening was mitigating warming"*

COMMENT: The statement oversimplifies the global impact versus regional impacts of greening. The statement, based on a model simulation with a single version of a single Earth System Model, implies that the effect of greening (i.e. darkening of the surface) is to cause global cooling, but any potential cooling is limited to specific regions where plants cool through large water fluxes, primarily the tropics (Bonan 2008). Importantly, in the boreal region where most observed greening has occurred, greening can contribute to enhanced warming due to the lower albedo of boreal forests compared to tundra or snow (Betts, 2000; Y. Li et al., 2023). Cooling of tropical and warming of boreal systems has been confirmed using satellite observations by (Forzieri et al., 2017).

ACTION: The authors need to add additional discussion about the expected climate impacts of leaf area increase in different regions, in particular that darker surfaces in high latitudes cause additional warming (G. B. Bonan, 2008).

Page 3

Comment Type: Technical

DOE CWG STATEMENT (second paragraph of section 2.1.1, page 3): *"Piao et al. (2020) noted that greening was even observable in the Arctic."*

COMMENT: This statement implies that the Arctic greening signal was caused by elevated CO_2, however that is not the scientific consensus. Piao et al. (2020) attribute the greening trend in the Arctic predominantly to growing season length driven by warmer temperatures (see also Y. Zhang et al., 2022). Piao et al. (2020) also note that this positive impact of increasing temperatures appears to have weakened over the past four decades, "suggesting a possible saturation of future greening in response to warmer temperature" (see also comment on greenness trends related to Section 2.1.1, first sentence of Page 4). It is also important to put Arctic greening more broadly into the context of the carbon cycle and other impacts. While above-ground plants may have displayed more leaf area over the past decades, rising temperatures also thaw permafrost and drive accelerated decomposition in highly carbon rich soils (Turetsky et al., 2020), a process which is expected to accelerate as climate continues to warm (Miner et al., 2022). Thus even with Arctic greening, high latitude terrestrial systems may become net carbon sources to the atmosphere, causing an amplifying feedback (Braghiere et al., 2023). Other risks to the Arctic linked to higher CO_2 levels and rising temperatures are not mentioned in this report (Virkkala et al., 2025). The Arctic is warming at a rate of 2 to 3 times the global average, leading to thawing of permanently frozen soils (permafrost), with downstream impacts including loss of structural support for buildings and subsidence, threatening communities, roads, runways, and other assets across Alaska (Manos et al., 2025; University of Alaska Fairbanks Institute of Northern Engineering US Army Corps of Engineers Alaska District & Laboratory, 2019).

ACTION: The report authors need to:

- add additional discussion of the role of temperature and other global change factors on observed greening trends in the Arctic
- add discussion that beneficial effects of global change seen in some high latitude regions are likely to saturate and possibly even reverse
- add discussion of how Arctic greening is counteracted by accelerated decomposition, weakening the carbon sink and likely leading to a net carbon source inclusive of the references above.
- The potentially positive effects of Arctic greening should be counter-balanced by negative effects of Arctic warming including melting permafrost and threats to infrastructure.

Page 3

Comment Type: Technical

DOE CWG STATEMENT (last paragraph of section 2.1.1, page 3): "*While plant models predict increased photosynthesis in response to rising CO_2, Haverd et al. (2020) reported a CO_2 fertilization rate much larger than model predictions. That is, CO_2 fertilization had driven an increase in observed global photosynthesis by 30 percent since 1900, versus 17 percent predicted by plant models. If true it would indicate that global models of the socioeconomic impacts of rising CO_2 have understated the benefits to crops and agriculture."*

COMMENT: The statement is incorrect for multiple reasons. First, the Haverd et al., (2020) study does not focus on agriculture, but on the net carbon exchange between the global biosphere and the atmosphere.Thus it does not support any statements about the response of crops or agriculture specifically. Second, Haverd et al., (2020) also uses a prediction from the CABLE land surface model to attribute how much of the historical change in gross primary production was likely caused by increasing CO_2. Their predictions are thus subject to similar uncertainties to other models used for this purpose. It is therefore incorrect to say that the estimate is "larger than model predictions" because the estimate from Haverd et al. (2020) is also a model prediction. Further, while the Haverd et al. model prediction is higher than the mean, it is still within the range of GPP responses to historical CO_2 change predicted by the ensemble of carbon cycle models to which it is compared. So it does not actually represent a deviation beyond the range represented by existing approaches. It is also relevant that much of the global increase in GPP estimated using the model of Haverd et al., (2020) comes from tropical forests, but the long-term ecosystem scale CO_2 responses of these forests remains unknown, in the ongoing absence of the CO_2 manipulation experiments which would be necessary to determine this (Amigo, 2020). Many studies have predicted that the low phosphorus concentrations of soils in the Amazon in particular (Quesada et al., 2010) are likely to limit this response (Cunha et al., 2022; Fleischer et al., 2019). More recent literature that the DOE CWG cites (Keenan et al., 2023) argues for a lower CO_2 fertilization response of photosynthesis. Finally, these estimates of high rates of CO_2 fertilization and greening have not yet been reconciled with recently available remotely sensed estimates of trends in above ground biomass estimated from tree height which show little increase over the past two decades (L. Xu et al., 2021).

With respect to crop model performance, many studies have concluded that crop models underestimate the negative impacts of climate extremes on crop production (Barriopedro et al., 2023; Heinicke et al., 2022; Kornhuber et al., 2023; C. Zhao et al., 2017). Accordingly, focusing on a single outlying model result gives a misleading picture of their uncertainties.

ACTION: The report authors need to remove the incorrect statement that "Haverd et. al (2020) reported a CO_2 fertilization rate much larger than model predictions". If they choose to discuss Haverd et al. (2020) they should include the caveats outlined above. The final sentence "if true…" should be removed given that it is unreasonable to extrapolate from the results of a single study, especially given that the results don't fall outside of the range of prior estimates.

Page 4

Comment Type: Technical

DOE CWG STATEMENT (Section 2.1.1, first sentence on Page 4): "*Piao et al. (2020) and Chen et al. (2024) report that the greening trend continues with no evidence of slowdown, and* CO_2 *fertilization remains the dominant driver*"

COMMENT: This statement is incorrect. Piao et al. (2020) conclude that although "vegetation greenness is increasing at the global scale, the changes vary considerably between regions and seasons." This is not the same as saying there is no evidence of a slowdown. And in fact, Chen et al. (2024) found that a trend of increasing drought stress slowed the rate of greening from 2001 to 2020. Also, other studies — already mentioned in our comment on the first paragraph of section 2.1.1, page 3 — find a slowing down or reversing of the greening trend in many regions (Winkler et al., 2021; Yuan et al., 2019).

Moreover, the possible drivers of greening are the CO_2 fertilization effect, warming temperatures, land use change, and nitrogen deposition, and the dominant driver depends on the region. While Piao et al. (2020) attributed global trends in LAI to CO_2, warming from climate change has a positive effect in northern high latitudes (>50N) but a negative effect in the southern hemisphere extratropics.

ACTION: The authors should replace this statement with a discussion that more accurately reflects the established literature and current state of knowledge on trends and drivers of greening, including references mentioned here and in our previous comment.

2.1.2. Photosynthesis and CO_2 levels

Page 4-5

Comment Type: Technical

SUMMARY OF COMMENTS IN THIS SECTION: Section 2.1.2 suggests that plants (especially C3 plants) will benefit from additional CO_2 in the atmosphere. We agree that leaf-level photosynthesis increases under ideal conditions under elevated CO_2, but this section (like the entire report) neglects a large body of evidence that warming, drought, and nutrient limitations ultimately lead to reductions in photosynthesis in experimental studies. These negative effects have been observed in several regions across the globe.

ACTION: This section needs to be revised to include the rich body of literature that has investigated the impact of CO_2-driven climate change on ecosystems.

Page 4

Comment Type: Technical

DOE CWG STATEMENT: "*Over geological time the Earth's atmospheric* CO_2 *levels were usually many times higher than they are today. About 400 million years ago* CO_2 *levels were an*

estimated 2,000-4,000 ppm and were at or above 1,000 ppm for much of the interval from 200 to 50 million years ago (Berner 2006, Judd et al. 2024). Over the past 35 million years the level of atmospheric CO_2 *has been steadily declining, falling to as low as 170 ppm during glaciations (Gerhart and Ward 2010). While the modern rate of change in* CO_2 *may be high compared to prior intervals, the geological evidence is that plants and animals evolved under much higher* CO_2 *levels than at present*."

COMMENT: This section is misleading and not relevant for evaluating if anthropogenic climate change poses risks to plants, animals, and human beings. For one, many plants (C4 and CAM) and extant terrestrial mammals, including modern humans, evolved under CO_2 concentrations that did not exceed ~550ppm ($CENCO_2PIP$, 2023). Further, this statement diminishes the relevance of the rate of change of CO_2 and associated warming which is unprecedented in the fossil record. The closest comparable situation in the fossil record is the Paleocene-Eocene Thermal Maximum (PETM) when there was a fast injection of CO_2 into the atmosphere with rapidly warming climates (Tierney et al., 2022). This rapid rise of CO_2 coincides with mass extinctions in the ocean and reshuffling/migrations of organisms on land (Huurdeman et al., 2020; Zachos et al., 2001).

ACTION: The report needs to add a discussion of the relevance of the rate of change we are experiencing in CO_2 levels and CO_2-induced warming to ecosystems and organisms, or remove the statement entirely.

Page 4

Comment Type: Technical

DOE CWG STATEMENT (section 2.1.2, last paragraph on page 4): *'Had atmospheric* CO_2 *levels continued declining, plant growth would have declined and eventually ceased'*

RESPONSE*:* This statement is a) uncited b) not supported by theory, as continued drawdown of CO_2 depends on the continued growth of plants, c) inapplicable to the Earth system as we know it because over the past 485-million years, atmospheric CO_2 concentrations never dropped below ~180 ppm (Judd et al., 2024), and d) irrelevant to our modern-day situation where CO_2 is 430ppm.

ACTION: The authors need to remove this statement from the report.

Page 4

Comment Type: Technical

DOE CWG STATEMENT: "*Below 180 ppm, the growth rates of many C3 species are reduced 40-60 percent relative to 350 ppm (Gerhart and Ward 2010) and growth has stopped altogether under experimental conditions of 60—140 ppm* CO_2*. Some C4 plants are still able to grow at levels even as low as 10 ppm, albeit very slowly (Gerhart and Ward 2010).*"

COMMENT: The CO_2 levels cited in this statement are inapplicable to conditions experienced by the Earth over the past 485-million years. Over the past 485-million years, atmospheric CO_2 concentrations never dropped below ~180 ppm (Judd et al., 2024).

ACTION: The authors need to either remove discussion of plant behavior at CO_2 levels below ~180 ppm or they need to add in the context that these CO_2 levels are below anything experienced by the Earth over the past 485-million years, and thus are of little relevance to our modern day situation.

Page 5

Comment Type: Technical

DOE CWG STATEMENT(section 2.1.1, last paragraph page 5): "*Over the past 60+ years there have been thousands of studies on the response of plants to rising* CO_2 *levels. The overwhelming theme is that plants, especially C3 plants, benefit from extra* CO_2*. There are two mechanisms by which* CO_2 *confers a growth benefit:*

- *Enhanced photosynthesis via the metabolic pathways described above.*
- *Increased water use efficiency. This arises because plants draw in* CO_2 *by opening the stomata (pores) on the leaf surface. When* CO_2 *is scarce the stomata must be kept wide open for long periods, allowing water to evaporate. Under enriched* CO_2 *conditions the stomata remain closed for longer periods, thus helping the plant retain water longer, and so increasing water use efficiency."*

COMMENT: It is, as the report notes, well known that when plants are exposed to elevated CO_2 under ideal conditions (i.e., adequate water and nutrients and optimal temperatures), leaf-level photosynthesis increases (G. Bonan, 2015; Ficklin & Novick, 2017; Grossiord et al., 2020; Q. Zhang et al., 2019). This well-established understanding is a foundational principle of models used to simulate the terrestrial carbon cycle and land carbon uptake (Farquhar et al., 1980) that inform the IPCC assessments (Arora et al., 2020; Blyth et al., 2021; Canadell et al., 2021). The same is true of enhanced water use efficiency, and the response of stomata to varying CO_2 underlying the modeled leaf stomatal conductance has been present in several generations of Earth system models (Ball et al., 1987; Blyth et al., 2021; LEUNING, 1995).

It is also widely acknowledged that CO_2 is likely driving an increase in land carbon uptake. IPCC AR6 notes that "Increasing global net land CO_2 sink since the 1980s is mainly driven by the fertilization effect from rising atmospheric CO_2 concentrations (Fernández-Martínez et al., 2019; O'Sullivan et al., 2019; Sitch et al., 2015; Tagesson et al., 2020; Walker et al., 2021)(*medium confidence*)".

This outcome, as expected from theory, is not surprising. But it is also known that the response of ecosystems to long-term high CO_2 exposure is complex. Free-Air CO_2 Enrichment (FACE) experiments, which are the gold standard in simulating and understanding real-life ecosystem-scale responses to elevated atmospheric CO_2 (Hendrey & Kimball, 1994), have shown that when conditions are not ideal — nutrients are limiting, temperatures are warmer

than optimal, water availability is inadequate — the growth enhancement from elevated CO_2 for both crops and non-agricultural ecosystems often diminishes or disappears (Ainsworth & Long, 2021; DOE, 2020). DOE has carried out ecosystem-scale FACE experiments on a subset of non-agricultural ecosystems. One (ORNL FACE) showed an initial increase in net primary production (NPP) due to elevated CO_2 that then disappeared due to progressive nitrogen limitation, and another (Duke FACE) maintained increased rates of NPP over the duration of the experiment. An arid ecosystem (Nevada Desert FACE) only showed CO_2-induced NPP increases in wet years (DOE, 2020). It is thus anticipated that for some ecosystems, nutrient limitation in particular will constrain the degree to which CO_2 can enhance plant growth (Davies-Barnard et al., 2022; Goll et al., 2012; Terrer et al., 2019; S. Zaehle, 2013; Sonke Zaehle et al., 2011).

Long-term observations of forest dynamics suggest that turnover and mortality rates have increased through time (Büntgen et al., 2019; Marqués et al., 2023; Maschler et al., 2022; O. L. Phillips et al., 2004). Given that these changes have occurred alongside both rising atmospheric CO_2 and climate change, attribution of the causes of changes in mortality rates is challenging (Esquivel-Muelbert et al., 2020; Oliver L. Phillips et al., 2009; Pillet et al., 2018) but growth-driven changes in turnover (the 'grow-fast, die-young' hypothesis) are supported by empirical findings (R. J. W. Brienen et al., 2020; Marqués et al., 2023), and align with ecological theory (Bugmann & Bigler, 2011). Given that most Earth System Models omit such trade-offs (Blyth et al., 2021; Fisher & Koven, 2020), projections of CO_2-driven carbon uptake may be overly optimistic (Marqués et al., 2023; Needham et al., 2022; Yao et al., 2024).

Additional consequence of rapid climate change include impacts on the viability of individual species. IPCC AR6-WGII Ch1 notes that *"Climate change increases risks to fundamental aspects of terrestrial and freshwater ecosystems, with the potential for species' extinctions to reach 60% at 5°C global mean surface air temperature (GSAT) warming (high confidence), biome shifts (changes in the major vegetation form of an ecosystem) on 15% (at 2°C warming) to 35% (at 4°C warming) of global land (medium confidence)".* Lapola et al. (2025) further argue that direct CO_2 fertilization can be considered a disturbance agent, via impacts on species composition and rainfall (via altered transpiration).

As CO_2 levels rise in the atmosphere, temperatures will concurrently rise. Plants will not experience elevated CO_2 concentrations independently of elevated global temperatures and consequentially altered climate regimes. In cooler regions, rising temperatures can enhance plant growth by extending the growing season, increasing photosynthetic rates, and accelerating nutrient cycling (Arft et al., 1999; Elmendorf et al., 2012; Rustad et al., 2001). However, these benefits are not universal. Way and Oren (2010) noted that while temperate and boreal trees often show increased growth with warming, tropical trees may not exhibit similar responses due to already operating near their thermal optimum. Higher temperatures can also negatively affect plant health by increasing respiratory carbon loss (Lloyd & Taylor, 1994); reducing plant carbohydrate reserves (J. Zhao et al., 2013); reducing photosynthetic efficiency (Didaran et al., 2024); and exacerbating water stress (Farquhar et al., 1980; Wahid et al., 2007). Heat stress can damage proteins and membranes (Jagadish et al., 2021), disrupt enzyme

function, and impair reproduction, leading to reduced growth and yield (Bita & Gerats, 2013; Lobell & Gourdji, 2012) and elevated plant mortality (Haberstroh et al., 2022). Extreme or prolonged warming can also exacerbate pest and pathogen pressure, further compromising plant performance (W. R. L. Anderegg et al., 2015; Bebber et al., 2013; Deutsch et al., 2018). Plants can partly acclimate to temperature by adjusting photosynthesis, respiration, and leaf traits (Atkin & Tjoelker, 2003; KATTGE & KNORR, 2007), but extreme temperatures can overwhelm this capacity, leading to carbon deficits and reduced performance (Slot & Kitajima, 2015; Way & Oren, 2010). Elevated temperatures have also been shown to negate the impacts of CO_2 fertilization on plant growth (C. Zhu et al., 2023). One of the longest running tropical forest monitoring studies, in Costa Rica, reports no CO_2-linked increase in forest productivity, and declines in forest growth during years with elevated nighttime temperatures (Clark et al., 2021).

Changes in terrestrial climate driven by elevated atmospheric CO_2 are also impacting plant water status via changes in surface relative humidity (Ficklin & Novick, 2017; Kim & Johnson, 2025; Willett et al., 2014) and rainfall patterns (Gebrechorkos et al., 2025; Kim & Johnson, 2025; Willett et al., 2014; OECD 2025) (See responses to section 6.7 on drought for further details of drought patterns). Drought and atmospheric drying decrease vegetation growth (Fu et al., 2022; Grossiord et al., 2020) and increase plant mortality (Allen et al., 2015; Bennett et al., 2015; Gazol et al., 2025; Gazol & Camarero, 2022; Hartmann et al., 2022; McDowell et al., 2022; Oliver L. Phillips et al., 2010; Senf et al., 2020), impairing the capacity of ecosystems to sequester carbon (William R. L. Anderegg et al., 2020; Canadell et al., 2021; Gatti et al., 2021; Hubau et al., 2020; Ke et al., 2024; Migliavacca et al., 2025; Peñuelas et al., 2017). The IPCC AR6 Cross Chapter Box 5.1 notes that “there is *high confidence* that the global net land CO2 sink is reduced on interannual scale when regional-scale reductions in water availability associated with droughts occur, particularly in tropical regions. There is also *high confidence* that the global land sink will become less efficient due to soil moisture limitations and associated drought conditions in some regions for high-emissions scenarios, specially under global warming above 4°C.” Impacts of climate change on fire are covered in a different section of this response but are also relevant here.

There is a growing body of evidence — from forest observation, remote sensing and atmospheric observations — that impacts of changing climate (temperature and drought) on plants are already having observable impacts on plant health and carbon uptake at continental scales, including the United States (Schwalm et al., 2012; Williams et al., 2023), the Amazon (R. Brienen et al., 2015; S. Chen et al., 2024; Gatti et al., 2021), African tropical forests (Hubau et al., 2020), the pan-tropics (Liu et al., 2023), China (Wang et al., 2025), and Europe (Forzieri et al., 2021; Fu et al., 2022; Segura-Barrero et al., 2025; van der Woude et al., 2023). Wu et al. (2025) and Wei et al. (2023) both find significantly intensified moisture controls over photosynthesis across the Northern Hemisphere and globe, respectively. Impacts of increased drought on global greenness trends was mentioned in our comments to Section 2.1.1 (X. Chen et al., 2024).

Earth system models, that simulate CO_2 fertilization effects, and atmospheric and soil moisture limitations on vegetation growth, predict that these impacts will substantially worsen under future climate scenarios (Cao et al., 2022; G. Chen et al., 2025; Cook et al., 2020; Julia K. Green et al., 2019; J. Li et al., 2024; C. Xu et al., 2019; Yang et al., 2024). Further, numerous lines of evidence suggest that the current generation of ESMs collectively underestimate numerous features of observed ecosystem drought responses (Bastos et al., 2018; J. K. Green et al., 2024; Kolus et al., 2019; Yin et al., 2025) and, while they capture CO_2 effects on photosynthesis, they typically do not include many other climate-driven hazards to plant growth and survival, including insect outbreaks (William R. L. Anderegg et al., 2015) , plant hydraulic mortality (Robbins et al., 2024), lagged effects of drought on plant health (Wei et al., 2022), complex interactions between plant phenology and frost events (Lambert et al., 2022), and seed dispersal limitations to plant adaptation (Zani et al., 2023).

In summary, while CO_2 can be considered to directly stimulate leaf-level photosynthesis, this does not mean that the net effects of rising CO_2 (inclusive of climatic change) will be to promote vegetation growth and carbon uptake over longer timescale, given the associated changes to climate variability, heat stress, and atmospheric and soil droughts, which are already visible and are anticipated to worsen. This is a very extensive and high profile area of research, which the authors of the CWG have chosen to ignore entirely, instead focusing extensively on a single study on leaf level CO_2 response.

ACTION: The report authors need to acknowledge and provide substantive discussion of the fact that rising CO_2 levels will co-occur with other changes to the Earth system – namely, warming temperatures and altered weather patterns – and that collectively these co-occurring changes will not necessarily result in a net benefit to global or even U.S. plant productivity as discussed in this comment. Their response needs include the references provided in this comment. Our review of this topic is based on more than 100 references beyond those cited in the DOE CWG report, along with a previous DOE report on the effects of elevated CO_2 on ecosystems. Each one of these references deal with one or more effects of temperature, CO_2, drought, and nutrients on photosynthesis and plants from greenhouse, experimental, and modelling studies.

2.1.3. Rising CO_2 and crop water use efficiency

Page 6

Comment Type: Technical

SUMMARY OF COMMENTS IN THIS SECTION: Section 2.1.3 purports to discuss rising CO2 and crop water use efficiency, but it bases the discussion primarily on papers that did not look at crops. Several inaccurate and misleading statements are made with regards to the cited literature, and a holistic review of the literature and topic of plant water use under CO2-induced climate change was omitted. We do not find justification for the section's conclusions about the risk of desertification, about any implications of global studies for crop yields, or about increased

water use efficiency. We recommended revising this section to rely on studies that explicitly consider crop water use efficiency. In this section (as elsewhere), the report's authors need to improve the accuracy of their summaries of cited literature and increase the number of studies they consider so that they accurately reflect current understanding.

Page 6

Comment Type: Technical

DOE CWG STATEMENT: "*They reported that models without CO_2 fertilization predicted CWP losses in every region, but those were more than offset by CO_2 fertilization so that all regions showed a net CWP gain.*"

COMMENT: The phrasing of this statement implies that the models used in the study were altered to consider CO_2 fertilization in comparison to no CO_2 fertilization. In reality, the study authors ran two different simulations, both of which modeled CO_2 fertilization. To quote the paper: "we considered two sets of simulations: the first accounting for both effects of elevated [CO_2] and changes in climate (CC w/ CO_2); the second accounting for changes in climatic conditions whilst keeping [CO_2] constant to present-day levels (CC w/o CO_2)." Thus, what changed was not the model but rather the model forcings. This is an important distinction because it demonstrates that the impact that elevated CO_2 has on water use by crops is already included in the models used by the IPCC and scientific community. It is not an aspect that has been overlooked or ignored, but included only by this one study. Instead, the study forcings were specifically designed to assess the impact of elevated CO_2 on simulated results using existing, standard models, which include CO_2 fertilization.

ACTION: Change the phrasing of this sentence to clarify that Deryng et al. (2016) does not report "models without CO_2 fertilization" but rather model simulations where CO_2 was kept constant compared to simulations where CO_2 was increased.

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Comment Type: Technical

DOE CWG STATEMENT: "*Deryng et al. (2016) also reported that negative impacts of warming on wheat and soybean yields were fully offset by CWP gains and mitigated by up to 90 percent for rice and 60 percent for maize."*

COMMENT: This statement reports only the median value found by Deryng et al. (2016), without acknowledging the large disagreement across the models considered in the paper. Between 60 and 80% of models agreed that Maize yield would decrease, while for rice and soybean less than 60% of models agreed on the sign of response of yield. Only for wheat did more than 60% of models predict that negative impacts of warming on yield could be offset. It is noteworthy that in the absence of rising CO_2, the yield changes simulated by the models tended to agree, while

yield changes simulated by the models with rising CO_2 were highly variable (see supplementary figure S6). This difference in agreement indicates that the plant response to elevated CO_2 remains uncertain. Additionally, these globally gridded crop models do not account for additional temperature driven factors impacting yield such increases in pest pressure which are predicted to have particularly large impacts on wheat (Deutsch et al., 2018). The scientific consensus is that CO_2-induced climate change—including rising temperatures, shifting weather patterns, and altered precipitation—will outweigh the benefits of CO_2 fertilization, leading to net yield declines across major agricultural regions in the U.S. and globally (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Muehe et al., 2019; C. Zhu et al., 2023).

ACTION: The report authors need to change this statement to clarify that the reported yield gains are the median response across the models and explicitly note that there was large disagreement among the models in terms of yield change. They also need to discuss other temperature driven negative impacts on yield that are not included in the globally gridded crop models analyzed by Deryng et al (2016) such as increasing pest pressure. They need to put the study in the context of the greater body of scientific understanding about how elevated CO_2 and CO_2-induced climate change will affect crop yields

Page 6

Comment Type: Technical

DOE CWG STATEMENT: *"Similarly, Cheng et al. (2017) noted that increased Gross Primary Production from 1982 to 2011 due to rising* CO_2 *uptake was accompanied by such large gains in CWP that global water use by plants had not increased, despite the extra biomass."*

COMMENT: Cheng et al. (2017) does not asses crop water productivity defined by the report authors as "yield per unit of water used". Cheng et al. (2017) asses water use efficiency of the bulk land surface with no evaluation or comparison with crop yields and thus the findings are not applicable for evaluating changes in CWP. Li et al. (2023) conversely found that global ecosystem-level WUE (which is what Cheng et al. 2017 is focused on) has not increased since 2001 because of the increases in atmospheric evaporative demand (VPD), and this stalling of changes in WUE will likely continue in the future because of continued increases in VPD due to climate change.

ACTION: The report authors should remove this statement from the crop water use efficiency section as the paper cited does not evaluate crop water use efficiency. If they choose to include a discussion of Chen et al. (2017) elsewhere the report authors should also discuss Li et al. (2003) and specify that while water use efficiency does increase under water stress, actual plant water use is also subject to increased demand from the atmosphere.

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Comment Type: Technical

DOE CWG STATEMENT: *"Deryng et al. (2016) assumed that climate change would "exacerbate water scarcity". Yet while models do predict that drylands will expand under climate warming, current data show the opposite: greening is happening even in arid areas. Zhang et al. (2024) report that due to increased* CO_2 *levels "increasing aridity in drylands won't lead to a general loss of vegetation productivity"; at most only 4 percent of currently arid areas will see increased desertification."*

COMMENT: First, again, this section is about *crop* water use efficiency, so it is misleading to highlight Zhang et al. (2024), which is about non-cropped regions.

Second, climate change indeed *has* already exacerbated water scarcity (and increased water demand) for agriculture and is projected to do so in the future (e.g. Chandanpurkar et al., 2025; Kinnebrew et al., 2025; Qin et al., 2020; Vicente-Serrano et al., 2022; Williams & Abatzoglou, 2025). Relying on studies that look at 'greening' is an insufficient measure of present or future water scarcity.

Third, Zhang et al. (2024) produce future projections of dryland vegetation productivity using regression relationships derived from the historical period. This is a flawed approach for projecting out of sample conditions in the future where temperatures have increased further, atmospheric dryness has increased, and the potential benefits of increased CO_2 have saturated (e.g. Grossiord et al., 2020; F. Li et al., 2023; Q. Zhang et al., 2019). Indeed, studies have shown an observed *decrease* in vegetation productivity in drylands due to increasing water constraints (Dannenberg et al., 2022; Jiao et al., 2021; Williams et al., 2023; Yuan et al., 2019).

ACTION: The report authors should remove these sentences. If they chose to discuss this topic, they need to bring in the body of literature cited in the comments to provide a more accurate depiction of topic and current understanding.

2.1.4. CO_2 fertilization benefits in IPCC Reports

Page 6

Comment Type: Technical

SUMMARY OF COMMENTS IN THIS SECTION: Section 2.1.4 suggests that the IPCC has neglected to report on global greening and CO_2 fertilization, but we explain how this is not true and direct the authors to remove misleading statements and refer to the IPCC's work on the global carbon cycle, which includes greening and CO_2 fertilization.

DOE CWG STATEMENT: "*The IPCC has only minimally discussed global greening and* CO_2 *fertilization of agricultural crops. The topic is briefly acknowledged in a few places in the body of*

the IPCC 6th and earlier Assessment Reports but is omitted in all Summary documents. …. Overall…the Policymaker Summaries, Technical Summaries, and Synthesis Reports of AR5 and AR6 do not discuss the topic."

COMMENT: The authors need to remove these misleading statements that imply the IPCC is intending to mask or hide the occurrence of "global greening" and "CO_2 fertilization." The IPCC AR6 report extensively covers the trend in land carbon uptake (the net amount of carbon leaving the atmosphere and being stored in land systems), of which CO_2 fertilization is only one of many competing factors. Land carbon uptake is a key component of the global carbon cycle; it is one of the major factors that control the amount of human released CO_2 that stays in the atmosphere. The DOE report's omission of discussion of net uptake of carbon by terrestrial ecosystems artificially excludes a large body of relevant literature. The role of elevated CO_2 in boosting land carbon uptake through enhanced photosynthesis is discussed by the IPCC in Chapter 5 in section 5.2.1.4, as well as in the summary of AR6 Ch5, but in these sections it is also weighed against the role of temperature, water stress, nutrient limitation, and changes in respiration. It is the total land carbon uptake (not just CO_2 fertilization) that determines the climate response to CO_2 emissions (described in IPCC AR6 Ch5 section 5.2.1.4.1). The expected trend of increasing but saturating land carbon uptake is described in the summary for policy makers (SPM section B.4.1) and noted in SPM Figure 7.

The Policymaker Summaries, Technical Summaries, and Synthesis reports are written to condense and reduce the jargon-heavy and technical main body of the IPCC report and highlight the most pertinent and relevant portions for policymakers. The fact that "global greening" and "CO_2 fertilization" are not explicitly mentioned in these summaries and synthesis reports reflects the fact that the specific impact of "global greening" and "CO_2 fertilization" were not relevant on their own for informing policies and decisions under consideration by policymakers. As previously discussed, global greening and CO_2 fertilization together are only one aspect of many competing factors that collectively determine the net effect that increasing CO_2 has on the land carbon sink, and trends in the net land carbon uptake are discussed in the Summary for Policy Makers (section B.4.1 and SPM Fig. 7). The IPCC reports are not hiding or masking the occurrence of global greening or the role of CO_2 fertilization. The text cited by the authors of this report note that the IPCC concluded "with high confidence" that greening has occurred, but that there is "low confidence in the magnitude of the trend."

ACTION: The authors need to remove the misleading statements that imply the IPCC is intending to mask or hide the occurrence of "global greening" and "CO_2 fertilization". The report authors also need to include discussion of how all of the components of the terrestrial carbon budget, not just plant photosynthesis, as well as the net uptake of carbon by terrestrial ecosystems will be impacted by CO2-driven climate change.

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Comment Type: Technical

DOE CWG STATEMENT: *"There are also brief mentions of CO_2 fertilization effects and improvements in water use efficiency in a few other chapters in the AR6 Working Groups I and II Reports."*

COMMENT: There is an entire cross working group box describing changes in water use efficiency driven by CO_2 (cross chapter Box 5.1), which is substantial and should not be classified as "brief".

ACTION: The report authors should specifically refer to this cross working group box and remove the world "brief", or remove this statement entirely.

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Section 2.2.1: Changing pH

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This is a **technical comment on Section 2.2.1**

Chapter 2 of the DOE CWG report addresses the topic of The Alkaline Ocean. The chapter contains critical omissions and factual errors, as noted below:

> Page 7, Paragraph 1, Sentences 2-4
> Page 8, Paragraph 3, Sentences 1-4
> Technical Comment
>
> The authors' comparison between past and current ocean pH levels is fundamentally flawed because it ignores a central aspect of ocean carbonate chemistry: the timescale of responses in pH and carbonate saturation to a change in carbon dioxide (CO_2) concentration. Comparing past and current pH requires distinguishing between long-term steady states and transient changes, as natural long-term changes in ocean pH have a fundamentally different impact on ocean chemistry than the rapid input of carbon from fossil fuels (see Hönisch et al., 2012; Zeebe et al. 2016). When ocean pH is lowered rapidly (e.g. over 10s to 100s of years), the abundance of carbonate ions decreases rapidly. These carbonate ions are the most critical ingredient for marine shell formation, and thus shells become harder to grow. Technically, this is known as a drop in calcium carbonate saturation state, and it makes calcium carbonate shell formation more energetically costly (e.g. Gagnon et al., 2021). In contrast, when ocean pH is lowered slowly (e.g. over 1000s of years or more), the dissolution of seafloor calcium carbonate, alongside weathering of rocks on land, has a buffering effect on ocean saturation state. This allows shell formation to continue unimpeded despite the lower pH. Indeed, the alkalinity balance (not just pH) helps determine whether $CaCO_3$ is stable. Over geologic time, weathering and dissolution can deliver enough alkalinity to keep the ocean saturated with respect to $CaCO_3$, allowing shells and reefs to form, even in a "high CO2, low pH" world (Kump et al., 2009).
>
> Today, anthropogenic CO_2 emissions are lowering ocean pH on decadal to centennial timescales, which are too rapid for these buffering processes to keep up with, resulting in lower calcium carbonate saturation states, and making shell formation more energetically costly and enhancing environmental stress on these organisms.

Consequently, the comparisons of Precambrian (Krissansen-Totton et al., 2018) and even the last deglaciation (Rae et al., 2018) ocean pH and marine life are *not* suitable analogs for understanding the effects of future ocean acidification, as has been demonstrated by the scientific community (Hönisch et al., 2012).

Page 7, Paragraph 1, Sentence 1
Technical Comment

The authors' discussion of "ocean acidification" as an incorrect term is additionally misleading.

As a linguistic point, a substance does not need to be acidic to undergo acidification, just as deforestation can occur in a region that is mostly forest, melting can occur for an ice cube that is not fully melted, or light dimming can occur in a room not fully dark. More substantively, neutral pH simply refers to the condition when hydrogen ions [H+] and hydroxide ions [OH−] are in equal concentration, although the pH where this occurs (e.g., 7.0) depends on other variables such as temperature, and it should not be viewed as a magic number in which a system's behavior suddenly changes. In humans, severe health complications can emerge for "low" blood plasma pH levels that are nonetheless higher than 7.0 (Jung et al., 2011). Similarly, disruption to carbonate chemistry critical for organisms like corals and shell-builders can occur for pH changes that do not cross the "7.0" threshold.

Acidification refers to an increase in hydrogen ion concentration ([H+]) regardless of whether the solution remains alkaline (pH >7.0) (Wang and Katz, 1965; Malan et al. 1985). This terminology is used outside climate science, for example in disciplines such as medicine, animal physiology, and plant physiology (e.g., Tarnow et al. 1975; Cheung et al. 1982; Malan et al. 1985; Raven, 1985), studies of changes to the pH of blood (e.g. Van Slyke, 1921),changes in pH of urine, bile, liver, heart, and pancreatic fluid (Malan et al. 1985). Note that these examples include studies that were published before ocean acidification was widely recognised. An alternative term used is acidosis (e.g. Wang and Katz, 1965). The statement's dismissal of "ocean acidification" ignores its widespread use and acceptance in scientific literature (e.g. Pörtner et al. 2004; Gattuso et al. 2015).

"Ocean acidification" became the more commonly used term for the phenomena of ocean carbonate chemistry changes because it provides a straightforward terminology for describing the declining pH of the ocean. It is not used in a way to indicate that the ocean is becoming an acid. In one of the first peer reviewed papers to document this process, Caldeira and Wickett (2003) describe both the process of declining pH as well as the context provided by the geologic record that indicates that the changes in ocean chemistry occurring today are large and very rapid compared to other times in Earth's history.

Page 7, Paragraph 1, Sentence 2
Technical Comment
The authors' claim that most ocean life evolved at a time when oceans were more acidic.

Comparison between past and current ocean pH also lacks fundamental context. The reference to Krissansen-Totton et al. (2018) shows that the oceans may have been more acidic when atmospheric CO2 concentrations were very high in Earth's early history, but there was no complex multi-celluar life (Knoll et al., 2011) and certainly no animals with skeletons requiring calcium carbonate (Knoll and Nowak, 2017). The Earth's ocean 2.5 billion years ago would be a very hostile environment to much of today's biodiversity, just as the atmosphere was also almost devoid of oxygen (Luo et al. 2016) and would be toxic to modern ecosystems. The earliest evidence of significant CaCO3 biomineralisation is 550 million years old (Murdock, 2020). Therefore, the pH conditions "in which life evolved", billions of years ago, is irrelevant for evaluating whether current or near-future conditions are suitable for modern ecosystems to continue (Pörtner et al. 2004; Halevy and Bachan 2017).

Modern marine life and ecosystems (e.g. coral reefs and open ocean fisheries) are very different from the initial lifeforms that evolved in the earliest oceans (e.g. bacteria, single-celled organisms), and so have different adaptive ranges and vulnerabilities. In the same way that the environmental conditions that were suitable for dinosaurs have little relevance for optimal conditions for humans, there is little relevance in comparing early ocean bacterial habitats to the conditions to which modern marine life has adapted. As discussed above, long term geological pH changes are not a good analogue for a rapid drop in pH of the type happening today, for the key reason that a rapid pH drop results in lowered calcium carbonate saturation state, which makes shell formation harder (e.g. Hönisch et al., 2012). To find better analogues for modern pH decline in the geological record, we need to look at times when ocean pH declined quickly, for example at the end-Triassic mass extinction (Trudgill et al., 2025). In fact, every major ocean mass extinction event for which we have pH reconstructions shows that a rapid drop in ocean pH accompanied the mass extinction event (Trudgill et al., 2025). The geological record thus suggests that rapid pH decline, as is occurring today, is a matter of serious concern for marine ecosystems and the communities and economies which they support.

Page 7, Paragraph 1, Sentence 3
Technical Comment
The use of "ocean pH" in this statement is misleading, as it seems to imply whole ocean pH or (following from the text above) the pH of the surface ocean, whereas the boron isotope study referenced here applies only to the deep Southern Ocean. These are completely different habitats occupied by completely different species. The habitats of most concern with respect to ocean acidification are in the surface (e.g. coral reefs, key

fisheries), where pH was higher than today during the last ice age as reconstructed by numerous studies (e.g. Hönisch and Hemming, 2005; Foster, 2008; Martínez-Botí et al., 2015; Ezat et al., 2017; Shao et al., 2019; Shuttleworth et al., 2021), not lower as suggested here. The authors must revise this text to clearly define the specific pH metric *and* the location to which it refers (whole ocean, surface ocean, deep ocean, etc.) and cite the appropriate reconstructions of that pH metric over geologic time.

The authors also state that "*Thus, ocean biota appear to be resilient to natural long-term changes in ocean pH since marine organisms were exposed to wide ranges in pH,*" which considering the scientific literature as a whole is a factual error. Several meta analyses have documented large scale and pervasive impacts of ocean carbonate chemistry change across multiple taxonomic groups, especially in marine calcifiers (e.g.,Kroeker et al., 2010; Bednarsek et al., 2019; Figuerola et al., 2021). Recent articles have also highlighted the persistence or tolerance of some species under declining pH conditions, and variability in responses to changing chemistry, both of which provide insights into potential for adaptation and marine biodiversity in the future (e.g., Hendricks et al., 2010; Chan and Connolly, 2013; Leung et al., 2022). In summary, a more balanced review of this topic would acknowledge that there are clearly negative consequences for some marine taxa, although perhaps not all. In addition, many of the taxa of concern are also commercially/economically valuable (for example, shellfish).

Technical Comment/ Omission

The only mention of the oceans throughout the entire report is in the context of ocean acidification, coral reefs, and sea level rise. The glaring omission of the myriad impacts of climate change on the ocean - marine heat waves, changing species distributions, changes in ocean circulation, increased harmful algal blooms, coastal erosion,and economic impacts on commercially valuable fisheries to name a few - is a significant problem with the report (see, for example: Wood et al., 2003; Fu et al., 2012; Mills et al., 2012; Le Bris et al., 2018; Gobler et al., 2020; Weiskopf et al., 2020; Paukert et al., 2021). To fix this, the authors should add a section describing these other impacts of climate change on the ocean.

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Response to DOE Section 2.2.2: Coral reef changes

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- **Page:** 7, section 2.2.2, paragraph one, sentence one
- **Comment Type:** Technical
- **Comment:**

The first sentence of the first paragraph of 2.2.2 states: "There are concerns that a decreasing pH of sea water will reduce the calcification rate of coral reefs".

This description fails to acknowledge that concerns that a decreasing pH of sea water will reduce the calcification rate of coral reefs are due to inferences based on significant and well established empirical evidence that decreasing pH is already reducing the calcification rate of coral reefs (Orr et al. 2005; Hofman et al. 2010; Barkley et al. 2015; Albright et al., 2018). A recent meta-analysis of 83 studies (1998–2020) quantifies a global decline in coral reef calcification rates averaging 4.3% ± 1.9% per year, directly linked to decreasing pH (from ~8.2 to ~8.04) and warming (Cornwall et al. 2021). It includes both field data from long-term monitoring (e.g., Great Barrier Reef, Caribbean) and experimental studies (Cornwall et al. 2021). It concludes that there is a low likelihood that the world's coral reefs will maintain their functional roles (e.g. coastline protection, hot spots of biodiversity, value for tourism) without near-term stabilization of atmospheric CO2 emissions.

- **Fix:**

This context should be included.

- **Page:** 7, section 2.2.2, paragraph one, sentence two
- **Comment Type:** Technical
- **Comment:**

The second sentence of the first paragraph of 2.2.2 states: "coral reefs already endure large swings in pH, partly due to daily photosynthetic activity in the reef; measured pH values range from 9.4 during the day to 7.5 at night (Revelle and Fairbridge, 1957).".

This statement implies that coral reefs will be resilient to current and future decreases in pH as they already experience variation in pH. However, this statement ignores the fact that these values are a single example taken from "reef pools" in Western Australia and were associated with changes in temperature and evaporation. The same sentence in the same reference (Revelle and Fairbridge, 1957: page 279) states that "the adjacent sea remained 8.2". Therefore, it is clearly inappropriate to imply that a range of 9.4-7.5 is representative of pH variation in coral reefs as a whole. More extensive recent studies conducted on the Hawaiian archipelago show that daytime variation in pH is less than 0.5 (Guadayol et al. 2014). Moreover, changes in overall ocean pH will shift the entire range of variation towards a lower range with a more extreme minima, a minima which will impact modern reef organisms such as corals and bivalves (Albright et al., 2018; Cornwall et al. 2021). Although specific studies have documented the resilience of corals in a few locations to changes in pH (e.g., Jury and Toonen 2019), the bulk of the scientific evidence does not support this type of resilience (Barkley et al., 2015). Also such resilience may not manifest when combined with increases in temperature (Guillermic et al. 2021). Regional variations in ocean pH do exist, e.g. due to variation in local upwelling (Great Barrier Reef, Caribbean, Indo-Pacific, Red Sea) but these tend to be associated with regional variations in calcification (Cornwall et al. 2021). Regions with lower pH also show lower rates of calcification (Cornwall et al. 2021). Regions with lower pH (e.g. CO_2 vent sites in Indo-Pacific) also exhibit lower calcification. As stated in Barkley et al (2015): "a comparison of the naturally low-pH coral reef systems studied so far revealed increased bioerosion to be the only consistent feature among them, as responses varied across other indices of ecosystem health."

- **Fix:**

This additional evidence and context should be included.

- **Page:** 7, section 2.2.2, paragraph one
- **Comment Type:** Technical
- **Comment:**

The final sentence of the first paragraph of 2.2.2 states: "But Ridd et al. (2013) showed that report to have resulted from a biased data analysis that, when corrected, showed no change in calcification rates. Nevertheless, the alarm produced by the original paper has persisted as evidenced by the large number of published citations (541) to the original study compared to only 11 citations to the correction (as of 30 April 2025)".

As a correction of fact: Ridd et al. (2013) actually has 20 citations according to Google Scholar (as of 14 August 2025). Moreover, some of these citations are by studies that directly criticise its inferences (De'ath et al. 2013: Schaffelke et al. 2018).

More importantly, although Ridd et al. (2013) did correctly identify the outermost bands of corals were incompletely formed, and therefore under-estimated, in De'ath et al. (2009), applying this correction only alters the estimate of the decline in calcification over the period 1990–2005 from 14.2% to 11.4%. The suggestion that the data shows "no change in calcification rates" is false and is based on erroneous assumptions about ontogenetic variation that aren't supported by independent observations (De'ath et al. 2013). The updated decline of 11.4% (0.76% yr− 1) is still relatively high (De'ath et al. 2013) and subsequently studies of Porites corals have found similar estimates ~13 % (e.g. Guo et al. 2020).

Importantly, comparison of annual calcifications among century-scale datasets shows that the frequency of coral growth anomalies has increased in multiple regions since the late 1990s (Lough and Cantin, 2014). Again, a recent meta-analysis of 83 studies (1998–2020) quantifies a global decline in coral reef calcification rates averaging 4.3% ± 1.9% per year, directly linked to decreasing pH (from ~8.2 to ~8.04) and warming (Cornwall et al. 2021).

- **Fix:**

This additional evidence and context should be included.

- **Page:** 7, section 2.2.2, paragraph two
- **Comment Type:** Technical
- **Comment:**

The second paragraph of 2.2.2 states: “The most recent annual summary of GBR conditions from the Australian Institute of Marine Science indicates that coral production has rebounded strongly (AIMS, 2023). Figure 2.4 shows the results of the AIMS surveys of hard coral cover, expressed as a percentage of the reef area.”

It has been previously documented that hard coral cover as a metric is a poor representation of ecosystem health or diversity (Bruno et al. 2009; Hughes et al. 2018a). High coral cover on the GBR is driven by fast-growing Acropora corals, which may mask the loss of other species (Hughes et al. 2018a).

The latest AIMS survey shows that, in 2025, 48% of surveyed reefs underwent a decline in percentage coral cover, 42% showed no net change, and only 10% had an increase (AIMS 2025). Reefs with stable or increasing coral cover were predominantly located in the Central GBR (AIMS 2025). Regional declines ranged between 14% and 30% compared to 2024 levels, with some individual reefs experiencing coral declines of up to 70.8% (AIMS 2025).

- **Fix:**

This additional evidence and context should be included.

- **Page:** 7, section 2.2.2, paragraph two
- **Comment Type:** Reference
- **Comment:**

The final sentence of the second paragraph of 2.2.2 states "the rebound has surprised some observers." without a single citation to support this statement.

- **Fix:**

There is no citation which supports this statement. One should be provided.

- **Page:** 8, Figure 2.4
- **Comment Type:** Technical
- **Comment:**

Figure 2.4 shows "hard coral cover, expressed as a percentage of the reef area". However, the data presented ends in 2023 when data up to 2025 are available (AIMS 2025). The two additional years worth of data should be included.

- **Fix:**

Please update the plot of data.

- **Page:** 8, section 2.2.2, paragraph 3
- **Comment Type:** Technical
- **Comment:**

The third paragraph begins "It is being increasingly recognized that publication bias (alarming ocean acidification results preferred by high-impact research publications) exaggerates the reported impacts of declining ocean pH."

This sweeping statement is supported with reference to one publication from 2016 (Browman, 2016; note that the pages for this publication are "529–536" not "1–536"). While this publication does state "As is true across all of science, studies that report no effect of ocean acidification are typically more difficult to publish" it also states that research on ocean acidification "has clearly matured" and openly calls "for a heightened level of organized (academic) scepticism to be applied". A more recent and more detailed review of the literature is available (Connell and Leung, 2023). This meta-analysis of 373 studies across 24 years, concludes that although there is a decline in negative effects of ocean acidification as more studies are conducted, this pattern is due to improved experimental designs, adaptation in some species, and a broader range of taxa studied. Overall, the effects of ocean acidification are still negative particularly on calcification in corals and bivalves, skeletal mineralization in fish, and fish behavior.

- **Fix:**

This additional evidence and context should be included.

- **Page:** 8, section 2.2.2, paragraph 4
- **Comment Type:** Technical
- **Comment:**

The fourth paragraph refers to meta-analysis (Clements et al., 2021) of the negative effects of ocean acidification on reef fish behavior. However, the report does not mention work done since which cautions against “throwing the baby out with the bathwater” (Esbaugh 2023) particularly given that fish also have to deal with the effects of ocean warming (e.g. Pistevos et al. 2017).

Furthermore, although the negative effects of ocean acidification on reef fish behavior may be less negative than previously inferred, there remains significant evidence that ocean acidification impacts the health of fish larvae (e.g., increased mortality, altered energy budgets) and the habitats of habitat-dependent species (e.g., coral reef fish) (Di Santo, 2019; Siegfried and Johnson 2023; Priest, et al. 2024). Also large scale experiments involving multiple levels of the food web (algae + herbivorous invertebrates + predatory fish) show that increases in pH and temperature disrupt ecosystem dynamics (Goldenberg et al. 2017).

- **Fix:**

This additional evidence and context should be included.

- **Page:** 7, section 2.2.2, paragraph 5
- **Comment Type:** Technical
- **Comment:**

The first and second sentence of the last paragraph of 2.2.2 states "In summary, ocean life is complex and much of it evolved when the oceans were acidic relative to the present. CO2 levels for more than 200 million years afterward were many times higher than they are today." This summary conclusion is not justified by the previous content. It is misleading in a number of ways.

Ocean life did evolve when oceans were acidic but this was over 2.5 billion years ago when the only life in existence was single celled cyanobacteria and early eukaryotes. There were no multicelluar animals (metazoans), and certainly no animals with skeletons requiring calcium (Knoll & Nowak, 2017). The Earth of 2.5 billion years ago would be unable to support much of today's biodiversity. Therefore, the pH conditions "in which life evolved", billions of years ago, is irrelevant for evaluating whether current or near-future conditions are suitable for modern ecosystems to continue (Pörtner et al. 2004; Halevy and Bachan 2017).

Corals and reefs have undergone several extinctions and diversifications over their geological history and much of this disruption may have been due to rapid changes in ocean pH (Kiessling and Simpson, 2011). There have been five major extinction events in the past 260 million years impacting reefs and four of those coincide with evidence for ocean acidification plus warming (Kiessling and Simpson, 2011):
1. Permian–Triassic (251 Ma)
2. Triassic–Jurassic (199.6 Ma)
3. Early Jurassic (183 Ma)
4. Paleocene–Eocene (55.8 Ma)
These disruptions include both variations in reef volume and significant shifts in taxonomic representation (Kiessling and Simpson, 2011; Pandolfi and Kiessling, 2014).

The content of this section mentions corals and fish focuses but beyond general statements ignores other organisms critical to reefs. It completely ignores the significant impacts of ocean acidification on bivalves (Pfister et al. 2016; Bullard et al 2021) which are important as both commercial seafood and for protecting shorelines (van der Schatte Olivier, 2020). In 2020, bivalve mollusks accounted for ~17 million tonnes of aquaculture production (21% of total aquaculture), valued at billions of USD annually (FAO 2022). Oyster and mussel reefs (bioherms) reduce coastal erosion and enhance resilience to storms (Beck 2011; Gentry et al. 2019; van der Schatte Olivier, 2020). Also ignored are pteropods, shelled organisms that are key components of ocean food chains (that involve commercially important fish such as herring and salmon) and appear particularly sensitive to ocean acidification (Bednaršek et al. 2021). A meta-analysis synthesizes data from 27 studies (2010–2021) and found a ~20–40% shell dissolution at pH ~7.8 (Bednaršek et al. 2021).

- **Fix:**
- This additional evidence and context should be included.

- **Page:** 7 and 8, section 2.2.2 as a whole
- **Comment Type:** Technical
- **Comment:**

This section on "coral reef changes" fails to mention the increase in frequency and severity of coral bleaching events due to increase in ocean surface temperatures. Indeed, the entire DOE report avoids addressing this topic despite there being numerous scientific studies that do (e.g. Hughes et al. 2018b; da Silva et al. 2025; Doherty et al. 2025). One large analysis of bleaching records (100 globally distributed reef locations) from 1980 to 2016 found that the median return time between pairs of severe bleaching events has decreased since 1980 and is now only 6 years (Hughes et al. 2018b). This result suggests that the interval between bouts of coral bleaching is too short for a full recovery of mature assemblages. This increase in bleaching frequency is mainly due to warmer sea surface temperatures (Hughes et al. 2018b). Indeed, NOAA just released an updated assessment (https://www.coralreefwatch.noaa.gov/satellite/research/coral_bleaching_report.php) of the current global scale bleaching event, which represents a new record for its impact, with an estimated 84% of corals impacted by extreme and prolonged heat. As a whole, this section leaves the impression that ocean acidification is the only threat from anthropogenic greenhouse gas emissions, whereas in fact ocean warming is the primary threat.

- **Fix:**
- The ongoing impacts of human-caused warming on global reefs should be discussed in this section, given its relevance to "coral reef changes".

- **Page:** 7 and 8, section 2.2.2 as a whole
- **Comment Type:** Technical
- **Comment:**

This section focuses on the impact of ocean acidification on coral reefs, but the impacts of anthropogenic climate change on marine ecosystems extend far beyond this arena, yet are not considered in this entire assessment. Chief among these impacts are marine heat waves, which have had a profound impact on ecosystem services and economies around the world. For instance, the economic toll of the 2013-2016 marine heat wave (‘The Blob’) that impacted coastal economies from Alaska to California ran into the hundreds of million dollars (Smith et al., 2021).

Fix:

The report should add a new section on the impacts of human-caused climate change on marine ecosystems, including coral reefs.

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Assessment of Chapter 3 “Human Influences on the climate”

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Executive Summary

This assessment critically reviews Chapter 3 of the DOE report, "Human Influences on the Climate," identifying key misrepresentations of climate science:

1. **Misrepresentation of Climate Scenarios**: The DOE report misrepresents IPCC climate scenarios as predictions, rather than "what-if" tools, and falsely claims emissions projections consistently overstated trends in past scenario exercises.
2. **Downplaying Human Influence**: The report unjustifiably downplays human impact on warming, overstating solar influence and falsely suggesting human emissions are merely additive to natural variability.
3. **Flawed Understanding of Carbon Sinks**: The DOE report shows a misleading account of carbon sinks, omitting crucial information on their future limitations, which fosters false complacency.
4. **Inaccurate Urban Heat Island Assessment**: The report incorrectly suggests urbanization significantly biases global temperature data, despite the consilience of evidence showing minimal impact from the Urban Heat Island effect.
5. **Selective Literature Review**: The DOE report selectively presents non-consensus views, failing to offer a balanced scientific discussion and leading to a distorted representation of climate science.
6. **Selective Presentation of Data**: The report’s treatment of atmospheric CO_2 concentrations omits relevant scientific data, and the misleading graphical choices undermine the report's claims of comprehensive and unbiased review, instead reinforcing a false narrative that downplays the scale of recent atmospheric changes and the degree to which those changes are attributable to human activities.

Introduction

This section critically reviews Section 3 ("Human Influences on the Climate") of the DOE report "A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate" (DOE CWG) by providing an assessment of summary statements, with reference to the wider chapter. It identifies key factual errors, instances of selective evidence presentation (cherry-picking), and issues with reference interpretation. We frame our assessment in sections, structured as responses to the key summary statements proposed by the authors of Chapter 3 of the CWG.

Our assessment highlights **the DOE CWG's misinterpretation of what scenarios are, how they are intended to be used, and how they have been used in assessments** such as those reported by the Intergovernmental Panel on Climate Change (IPCC). The IPCC's use of scenarios is an acknowledgment of the inherent unpredictability of long-term socio-economic trends. The DOE report ignores this foundational principle and instead selectively focuses on high-end emissions scenarios, like RCP8.5, portraying them as failed predictions, to argue that the risks of climate change are exaggerated [see discussion of RCP8.5 on **page 15 in the CWG**]. This selective focus not only misrepresents the IPCC's methodology but also fundamentally misunderstands the role of scenario analysis in navigating complex and uncertain future challenges, a standard practice in any form of robust risk assessment.

The DOE highlights critiques of the highest emission scenarios from prior assessments which now exceed observed emissions, and these concerns are valid but uncontroversial - given that these scenarios are now generally considered to be outdated projections of coal-intensive futures. In cases where researchers have used RCP8.5 as a simple prediction of future emissions - this critique is valid.

The report maintains, however, that "RCP8.5 was misused not only by individual researchers, but also by influential science agencies like the IPCC and the U.S. National Climate Assessment (USNCA)" [direct quote from **page 15** in the CWG**]**. In stark contrast, the IPCC has been consistently and explicitly clear throughout its assessment reports that **scenarios are not predictions. They are exploratory tools**—coherent and plausible "what-if" stories—designed to investigate a wide range of potential futures and the consequences of different societal choices, particularly under conditions of "deep uncertainty" where assigning probabilities to future human actions is not feasible.

The DOE report's treatment of atmospheric CO_2 concentrations further exemplifies its **selective presentation of evidence.** By focusing solely on Mauna Loa data since 1958 and manipulating visual representations [see Section 3.1.2 on **page 13** and Figure 3.1.3 on **page 14**], the report obscures critical historical context, such as the **dramatic acceleration of CO_2 growth rates since the mid-20th century**, the overwhelming dominance of fossil fuel emissions over land-use change, and the fact that current CO_2 levels are far outside the natural variability of the past 805,000 years. The omission of earlier scientific data and the misleading graphical choices undermine the report's claims of comprehensive and unbiased review, instead reinforcing a narrative that downplays the scale and human attribution of recent atmospheric changes.

Furthermore, **the report presents a fundamentally flawed understanding of natural carbon sinks,** misrepresenting their role in mitigating human-induced climate change. It misleadingly

compares human emissions to natural exchanges without acknowledging that human activities disrupt the system's long-term balance [see **page 18**, where the CWG states that human emissions are "only about 5 percent of the annual exchange"]. Crucially, **the report conflates the *absolute magnitude* of carbon uptake by land and ocean sinks with their *fractional uptake*,** ignoring that for effective mitigation, the fractional uptake would need to increase, and this increase is not occurring. The narrow focus on historical trends neglects substantial scientific literature on the future behavior and inherent limitations of these sinks, particularly the decreasing capacity of the ocean due to buffering and warming, as well as land sink constraints due to nutrient limitations and temperature effects. These omissions downplay the urgency of emission reductions and thereby foster a misleading sense of complacency.

Responses to Summary Statements

Comment 1: On the Cause of Recent Warming

Reference in CWG Report: Page 11 (Summary Statement).

Comment Type: *Technical*. This addresses the scientific framing of human influence.

Summary statement from CWG Report: "The global climate is naturally variable on all time scales. Anthropogenic CO2 emissions add to that variability by changing the total radiative energy balance in the atmosphere".

Comment:
This statement is a *partial truth* that omits crucial context regarding the magnitude and primary driver of recent climate change. While natural variability exists on all timescales, the IPCC AR6 and NCA5 unequivocally state that human activities have caused the observed global warming since the industrial era. The increase in atmospheric CO2 and other greenhouse gas emissions from human activities is the dominant driver of the observed global surface temperature increase, not merely an additive factor to natural variability. For example, NCA5's (USGCRP, 2023) Key Message 3.1 directly states, "Human Activities Have Caused the Observed Global Warming", and Figure 3.1 in NCA5 explicitly attributes observed temperature change since 1850-1900 primarily to total human influence, with natural drivers showing a near-zero contribution. The Indicators of Global Climate Change (Forster et al., 2025) similarly tracks and attributes human-induced warming, noting that “generally, whatever methodology is used, on a global scale, the best estimate of the current level of human-induced warming is (within uncertainty) similar to the observed global surface temperature change.”

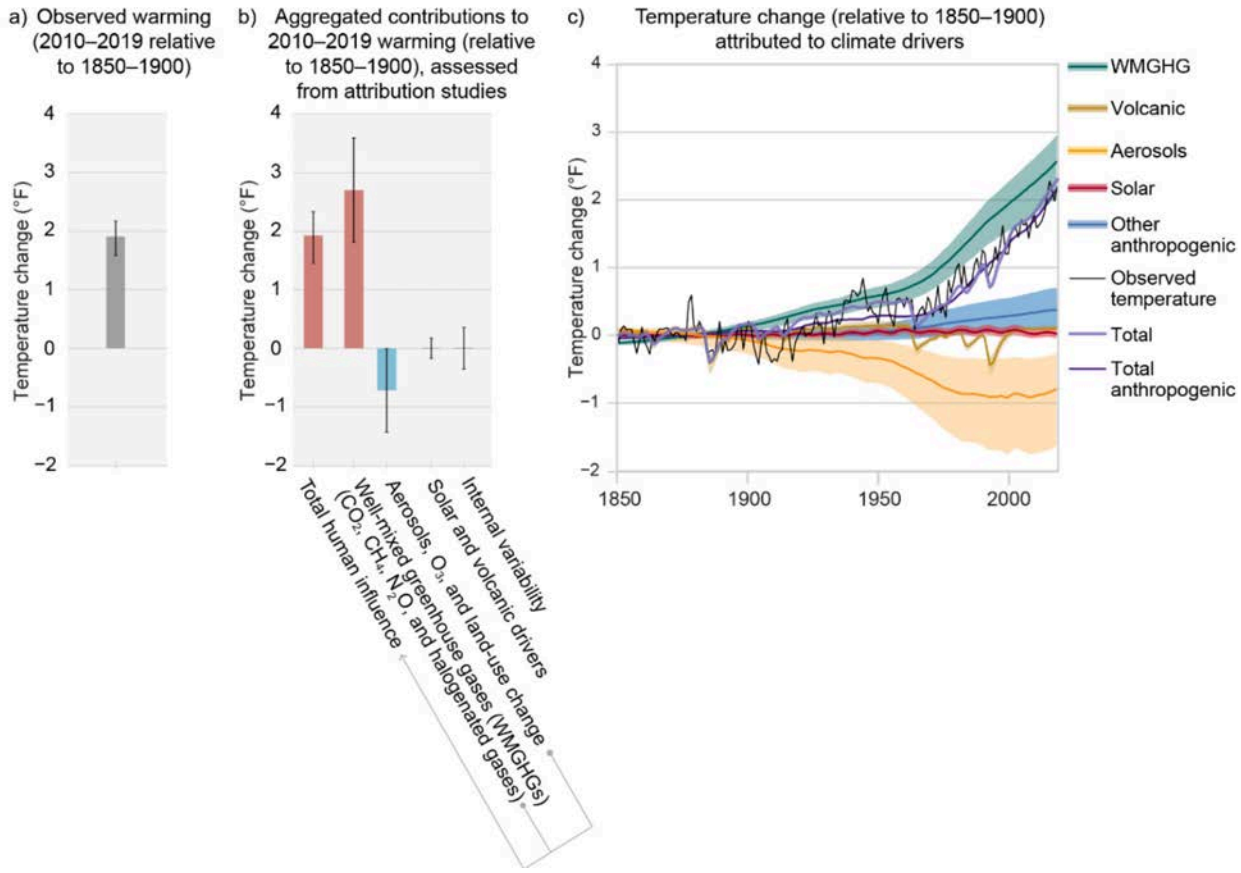

Figure 1: From** (USGCRP, 2023) **- showing (a) observed global average surface temperature change in 2010–2019 relative to 1850–1900; (b) temperature change over the same period attributed to total human influence, including greenhouse gases (CO2, CH4, N2O, and halogenated gases), aerosols, ozone (O3), and land-use change, as well as solar and volcanic drivers and natural climate variability; and (c) the time evolution of observed temperature (2010–2019, relative to 1850–1900; black line) attributed to different climate drivers from 1850 to 2019, including total human influence (purple line) and combined natural and human influence (lavender line). Whiskers in (a) and (b) show the very likely range, while shaded uncertainty bands in (c) show very likely (5%–95%) ranges.

Comment 2: On the Role of Natural Influences

Reference in CWG Report: Page 11 (Summary Statement); pp. 12-13 (Detailed Argument).

Comment Type: *Technical / Reference.* This comment challenges the original chapter's scientific conclusion and its selective use of sources.

Original Text/summary statement from CWG Report: "The IPCC has downplayed the role of the sun in climate change but there are plausible solar irradiance reconstructions that imply it contributed to recent warming".

Comment:
This summary statement misrepresents the scientific consensus on solar influence. The report highlights the work of (Connolly et al., 2021), which argues that the scientific consensus on climate change attribution is premature [see citation and discussion of (Connolly et al., 2021)) on **pages 12 and 13 in the CWG**.]. They contend that by selecting specific datasets—namely, a "high solar variability" reconstruction of the sun's output and a "rural-only" surface temperature record that shows less warming—one can conclude that most of the observed warming in the Northern Hemisphere since the 19th century could be attributed to natural solar activity. This approach is intended to demonstrate that the level of uncertainty in key climate data is high enough to undermine the IPCC's confident statements about human-caused warming. However, the statistical methods used in (Connolly et al., 2021) have been heavily critiqued. (Richardson and Benestad, 2022) found that their approach is highly sensitive to the order of analysis in the sequential regression used, and that the anomalously high solar influence seen in some datasets could be demonstrated to be an artifact of improper processing.

IPCC AR6 and NCA5 explicitly include solar and volcanic activity as natural drivers of climate change(Forster et al., 2023; USGCRP, 2023). In contrast to the DOE report, the IPCC's conclusions are not based on a single analysis but on a comprehensive synthesis of many studies across multiple, independent lines of evidence, including energy budget analyses (Loeb et al., 2021), climate model simulations (Gillett et al., 2021), and the physical "fingerprints" of warming such as a cooling stratosphere (Santer et al., 2017)). This robust, multi-faceted process leads the IPCC to conclude unequivocally that human activities are the dominant cause of observed warming, with the net contribution from natural drivers like the sun being negligible over the long term.

Both IPCC AR6 and NCA5 consistently conclude that natural forcings have had a negligible, near-zero, or even slightly cooling effect on global surface temperature since 1850-1900, particularly when compared to the warming from anthropogenic greenhouse gases. NCA5 Figure 3.1c illustrates that the contribution from solar and volcanic drivers has been minimal compared to anthropogenic factors over the industrial era. The most recent Indicators of Global Climate Change paper (Forster et al., 2025) further supports this finding, showing that natural forcings contributed only 0.05 °C [–0.1 to 0.2] °C to observed warming in 2010–2019, while anthropogenic forcings contributed 1.09 [0.9 to 1.3] °C (see Figure 2). The DOE report's citation of (Connolly et al., 2021) as implying the IPCC's consensus on solar forcing was "formulated prematurely through the suppression of dissenting scientific opinions" [direct quote from **page 13** of the CWG] is an instance of cherry-picking a non-consensus view and misrepresenting the thoroughness of IPCC assessments. IPCC reports consider a wide range of published literature, and their conclusions reflect the overwhelming weight of evidence.

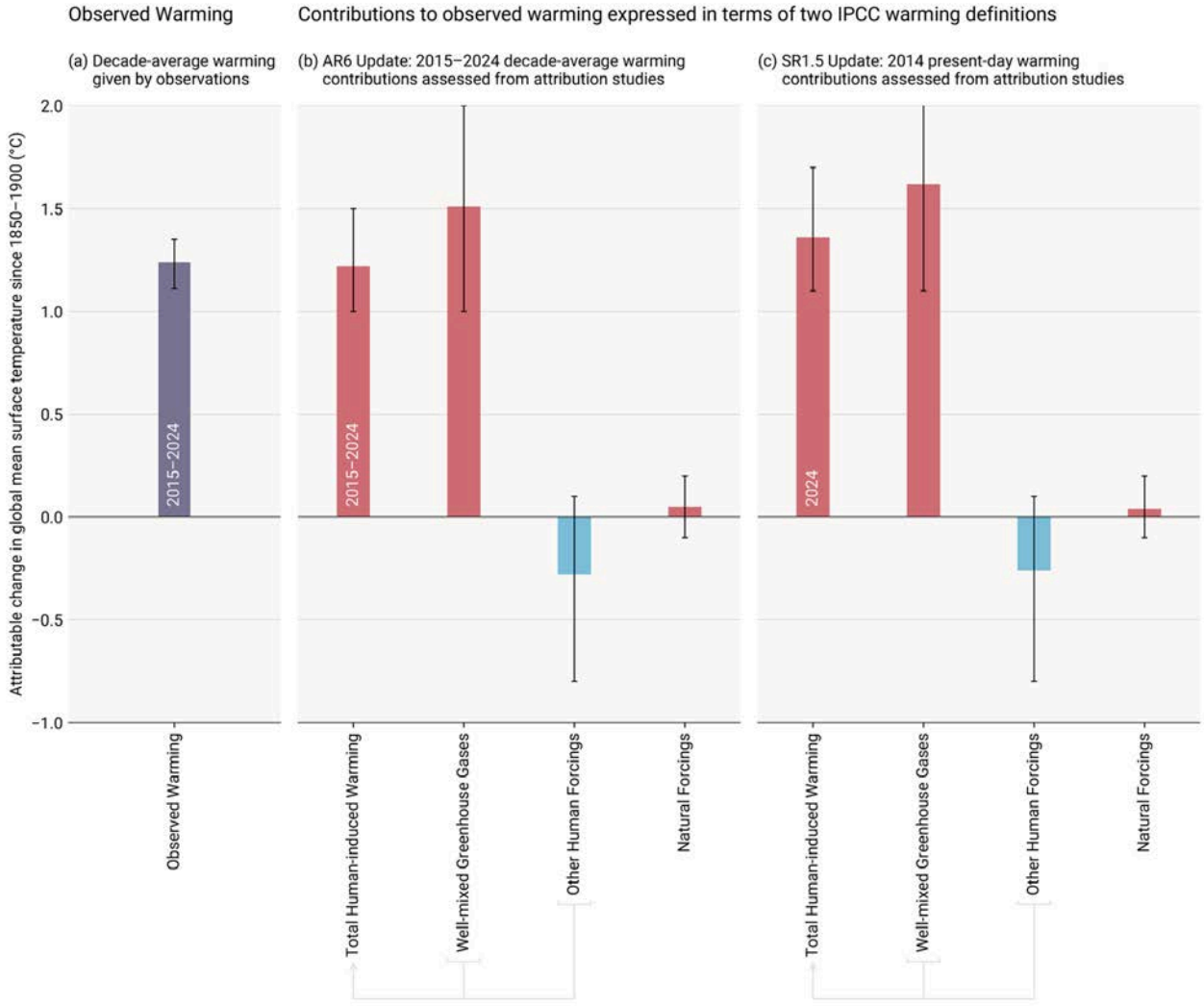

Figure 2 - from** (Forster et al., 2025), **showing the assessed contributions to observed warming relative to 1850–1900. The 2015–2024 average and 2024 results are this year's updated assessments for AR6 and SR1.5, respectively. Panel (a) shows updated observed global warming from Sect. 7, expressed as total global mean surface temperature (GMST), due to both anthropogenic and natural influences. Whiskers give the "very likely" range. Panels (b) and (c) show updated assessed contributions to warming, expressed as global mean surface temperature (GMST), from natural forcings and total human-induced forcings, which in turn consist of contributions from well-mixed GHGs and other human forcings. Whiskers give the "likely" range.

Comment 3: On the Use of Climate Scenarios

Reference in CWG Report: Page 11 (Summary Statements); pp. 14-16 (Detailed Argument).

Comment Type: Technical. This addresses the report's misinterpretation of the purpose of climate scenarios.

Original Text/summary statement from CWG Report: "Climate projections are based on IPCC emission scenarios that have tended to exceed observed trends. Most academic climate impact studies in recent years are based upon the extreme RCP 8.5 scenario that is now considered implausible; its use as a business-as-usual scenario has been misleading."

Comment:
This statement is an example of **selective presentation of evidence and misrepresentation of scenario purpose**. While it is true that some past high-end emission scenarios, like RCP8.5, have been critiqued for representing their high emissions pathways as a "business-as-usual" assumption (Hausfather and Peters, 2020a, b), the DOE report fails to accurately convey the nuanced role of high-emissions scenarios in climate science. The CMIP6 ScenarioMIP (O'Neill et al., 2016), which informed the IPCC-AR6 assessments, explicitly introduced **"**scenarios describing **possible** future developments of anthropogenic drivers of climate change**,"** which, as such, are not forecasts. High-end scenarios explore the possible impacts of tipping points and how large the climate change risks to which society may have to adapt might be.

Each generation of ScenarioMIP has included several scenarios, one of which in each case represents the upper plausible bound to inform the 'worst-case' scenario for climate adaptation. This upper bound shifts as understanding evolves. For the CMIP5 effort which informed IPCC-AR5, this scenario was RCP8.5 (Riahi et al., 2011) but the assessment of the upper bound changes with each cycle of CMIP and IPCC reports, as technologies, economic factors and geopolitical influences evolve. The high-end scenario in ScenarioMIP CMIP6, SSP5-85 was described as "the high end of the range of future pathways in the IAM literature" (O'Neill et al., 2016), with a similar end-of-century forcing level as RCP8.5 but with a larger role for methane relative to CO2. Current discussions for the next generation of scenarios for CMIP7 (van Vuuren et al., 2025) acknowledge that that "the plausibility of the CMIP6 high emission levels (quantified by SSP5-8.5) has been questioned" — and they therefore plan to use a high emissions scenario which is "expected to result in forcings below SSP5-8.5" — but at the same time these discussions highlight why high-end scenarios remain a necessary part of a wider set of plausible futures: because even at lower forcings they "explore the risk of reaching possible tipping points ... and how large might the climate change risks be to which society may have to adapt." Thus have the CMIP and IPCC processes responded to changing consensus on the upper plausible bound of future climate change, which itself evolves with changing assessments of energy systems and global socioeconomic conditions.

The claim that "IPCC emission projections have tended to overstate actual subsequent emissions" [page 15 of the CWG] misrepresents the range of emissions scenarios used in the ScenarioMIP process which informed IPCC-AR6. Global emissions continue to grow (Forster et al., 2025; Global Carbon Project, 2024), even if the rate of increase has slowed recently. For the CMIP6 scenarios (O'Neill et al., 2016), current CO2 emissions remain approximately in line with SSP2-45 and SSP4-60 (scenarios which do not diverge until the 2030s, see Figure 3). There were

two widely used scenarios significantly above current 2024 levels (SSP5-85 and SSP3-70), and two widely-used scenarios significantly below current CO2 emissions levels (SSP1-19 and SSP1-26). As such, observed emissions are well centered in the ScenarioMIP CMIP6 distribution.

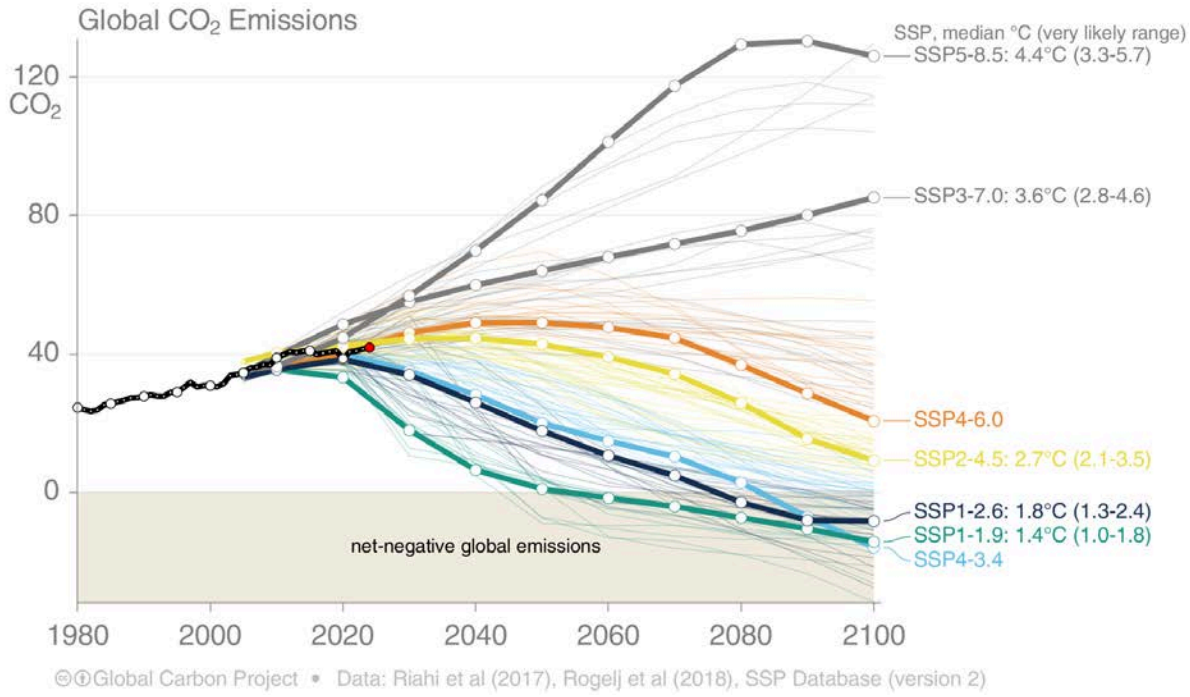

Figure 3: observed carbon dioxide emissions** (Global Carbon Project, 2024)**, black line with 2024 emissions shown as the red filled circle, in the context of CMIP6 scenarioMIP scenarios** (O'Neill et al., 2016)**. Figure prepared as supporting material for the 2024 Global carbon project** (globalcarbonproject.org, 2025)**.

Figure 3, above, differs notably from Figure 3.2.2 in the DOE report. Figure 3.2.2 shows only fossil CO2 emissions and excludes land use, but it is inaccurately characterized in the DOE report as "total global observed CO2 emissions" [figure is on **page 17 of the CWG**, and the inaccurate reference is in the text on **page 16**.]. Both land use and fossil CO2 emissions are important in determining the magnitude of future warming. Furthermore, agreement or disagreement between scenarios and observed emissions over the past decade is a limited constraint on the evolution of future emissions over the remainder of the century (Hausfather and Peters, 2020a, b), which depends on both technological costs and policy decisions.

Similarly, the DOE report's Figure 3.2.1 is copied from Figure S4 in (Hausfather et al., 2020) and purports to show that "observed atmospheric CO2 concentrations tracked the low end of the SRES range and also of subsequent IPCC scenario ranges". However, **the figure does not show what is being claimed in the DOE report.** Figure 3.2.1 (S4 in (Hausfather et al., 2020)) includes future

CO2 concentrations projected by 17 climate models published between 1970 and 2001. Only one of the lines – the one in red representing the A2 scenario in the IPCC TAR report – represents a SRES scenario, and it is broadly consistent with observed concentrations in the years after it was published. Earlier climate models that tended to overstate future CO2 concentrations, but this overstatement was counterbalanced by a lack of non-CO2 GHGs (e.g. CH4, N2O, halocarbons), such that the increase in total forcing in early models is generally lower than in observations (Figure 1 in (Hausfather et al., 2020)).

With respect to total CO2 emissions (including both best estimates of land use emissions and fossil emissions - as shown in Figure 4a): through 2015, CO2 emissions were on the high end of the range of SRES scenarios, but over the past decade they have moved toward the middle of the range as emissions have plateaued. In 2024, total CO2 emissions remained below those of the original RCP8.5, but above RCP4.5 and RCP6.0. Fossil-only CO2 emissions approximately track RCP4.5 (Figure 5b). In terms of SRES scenarios, fossil emissions remain above B1, B2, and A1T but below A1F1, A2, and A1B.

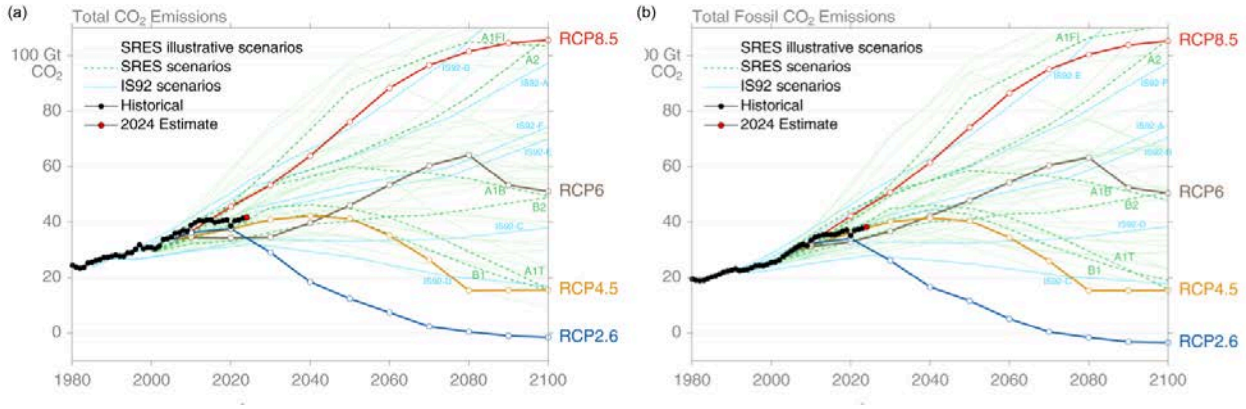

Figure 4: (a) Fossil fuel only and (b) total (fossil+land use) CO2 emissions in old scenarios (IS92, SRES, and RCPs) compared to observations, Updated from *(Peters et al., 2013)* ***with recent data from*** *(Global Carbon Project, 2024)* ***. Figure via Robbie Andrew and Glen Peters.***

Comment 4: On the Carbon Cycle

Reference in CWG Report: Page 11 (Summary Statement); pp. 17-19 (Detailed Argument).

Comment Type: Technical. This concerns the flawed and incomplete presentation of carbon-sink science.

Original text/summary statement from CWG: "*Carbon cycle models connect annual emissions to growth in the atmospheric CO2 stock. While models disagree over the rate of land and ocean CO2 uptake, all agree that it has been increasing since 1959."*

Comment:

This statement, and other similar statements in the report misleadingly downplay the magnitude of annual human-induced carbon emissions to the atmosphere by comparing them to the magnitude of natural exchanges with the ocean and land. Human emissions are quoted as being "only about 5 percent of the annual exchange with the atmosphere" [quote from **page 18 of the CWG]**. However, **this line of argumentation is entirely irrelevant to the long-term change in atmospheric carbon concentrations,** which is the salient quantity with regard to climate change. Natural exchanges are broadly in balance on an annual timescale (or multi-annual in the presence of inter-annual variability), meaning that they absorb the same amount as they emit, whereas human emissions knock the system out-of-balance. While imbalances can emerge in the natural exchanges (indeed, such imbalances in the ocean are thought to be the driver of ice age CO2 changes (Gottschalk et al., 2016)), there is no evidence that any such imbalance is playing a substantive or even contributory role in the present long term rise in atmospheric CO2 (Canadell et al., 2023; Global Carbon Project, 2024; Graven et al., 2017; Keeling et al., 2017). The distinction between the role of annual exchanges in the natural sinks and the long-term effects of human-induced emissions is most clearly seen by looking at the historical trend in atmospheric CO2, recorded at the Mauna Loa station (https://gml.noaa.gov/ccgg/trends/ - Figure 5), where it is evident not only that are CO2 concentrations increasing, but also that the amount of carbon added to the atmosphere each year has also increased over recent decades, with the rate of increase of atmospheric CO2 having risen noticeably from ~1.5ppm/yr from 1975-1999 to ~2.3ppm/yr from 2000-2024 (https://gml.noaa.gov/ccgg/trends/gr.html - Figure 5(NOAA Global Monitoring Laboratory 2024)). This highly significant and accelerating trend is misrepresented in the DOE report's Figure 3.1.3 [**page 14 of the CWG]**, which uses a limited timeframe and an inappropriate y-axis scale to visually downplay the clear increase in concentrations.

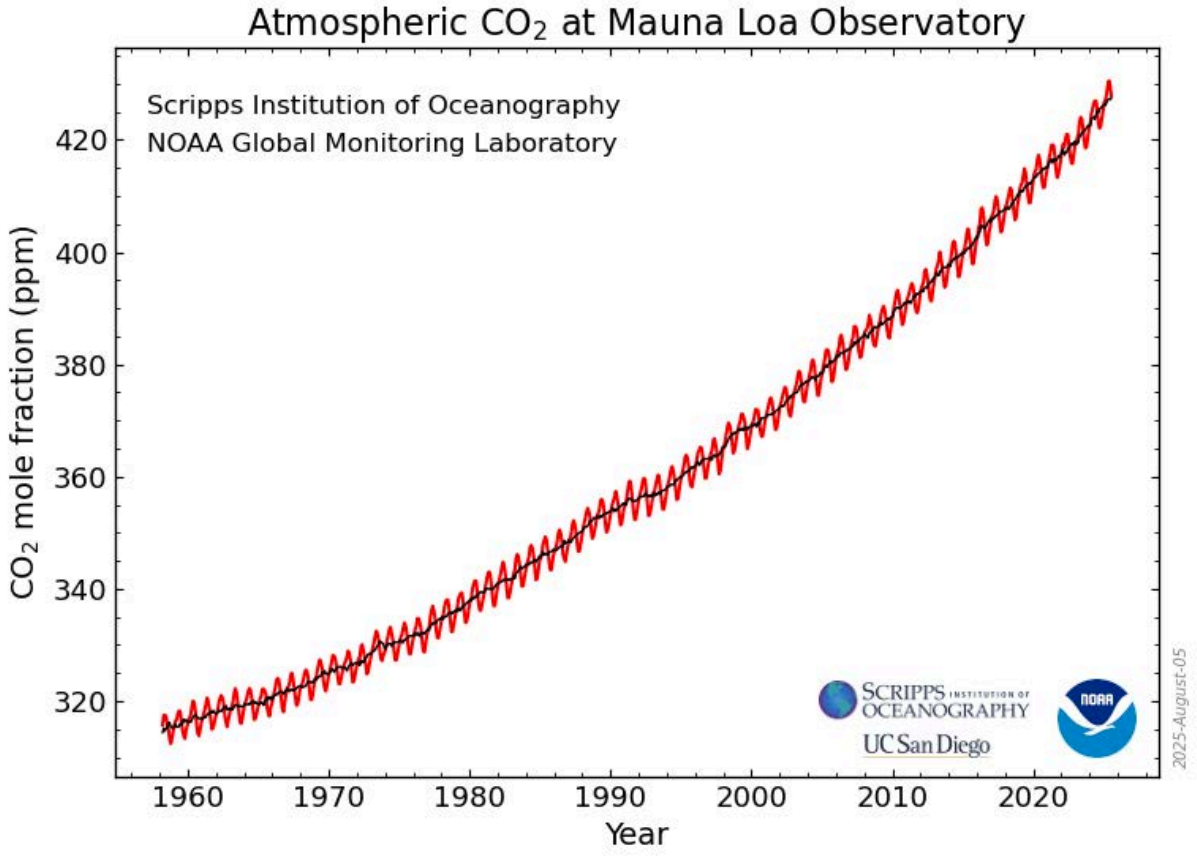

Figure 5: from** (NOAA Global Monitoring Laboratory, 2024)**, red line shows the monthly mean measured CO2 concentration at the Mauna Loa observatory in Hawaii, USA. The black line removes the seasonal cycle from the signal.

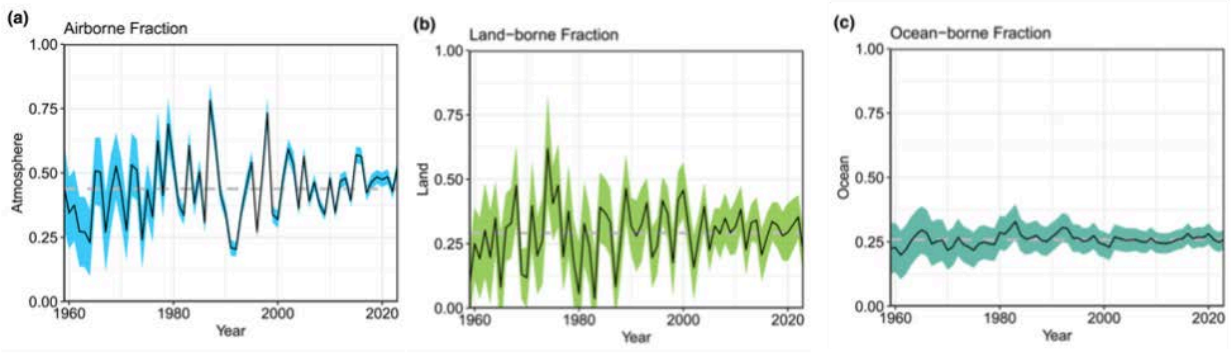

Figure 6 (from** (Global Carbon Project, 2024)**), showing the best-estimate of fraction of atmospheric CO2 emissions from fossil and land use sources which are accumulated in the atmosphere (a), land (b) and ocean (c). Black lines represent the central estimate, and the coloured shading represents the uncertainty. The dashed grey lines represent the long-term

average of the airborne (44 %), land-borne (30 %), and ocean-borne (25 %) fractions during 1960–2023.

The effects of annual natural exchange are evident as the cycles that occur around the long term increasing trend. The impacts of human emission on atmospheric carbon – in spite of large annual exchange with the ocean and land — are entirely unambiguous (Forster et al., 2025; Global Carbon Project, 2024; Masson-Delmotte et al., 2023), a finding supported by the evidence of isotopic ratios of carbon in the atmosphere which differ between living and fossil carbon sources (Graven, 2015). Fossil fuels are derived from ancient plant matter, which is naturally depleted in the Carbon-13 isotope and contains no radioactive Carbon-14, which is otherwise continuously formed in the upper atmosphere by cosmic rays (Lal and Peters 1967). Consequently, the observed decrease in the atmospheric ratio of both Carbon-13 and Carbon-14 serves as a definitive fingerprint of carbon from fossil fuel combustion (Graven et al. 2017). Debate on this point has been settled for decades (Lynas et al., 2021)

The report emphasizes that the ocean and land sinks have taken up around 50% of historical carbon emissions [**page 14** and **page 18** of the CWG], and that this fraction has remained approximately constant over time. There is little debate that while atmospheric CO2 levels increase, the planet's land and oceans respond by absorbing a portion of this CO2. Historical allocation of carbon emissions to the land (30 %), oceans (25 %) and atmosphere (44 %) has remained remarkably constant (Crisp et al., 2022; Global Carbon Project, 2024). The authors leverage this fact of historically constant airborne fraction to point out therefore that the land and ocean sinks must have increased in absolute magnitude. Therefore, they implicitly suggest, the natural sinks can, have, and will mitigate against rising emissions. This logic is fundamentally flawed and the framing critically misleading for several reasons:

1. **The quantity relevant to climate change via the concentration of atmospheric CO2 is the fraction of emissions taken up by the land and ocean, not the absolute magnitude of these sinks**, which the report tries to focus on. Consider the case of the industrial period: emissions increase and the fraction taken up by the land and ocean remains the same (i.e. 50%). While the absolute uptake by land and ocean has increased, so too has the amount remaining in the atmosphere (50% of the increased emissions). For the natural sinks to provide a mitigating effect to increasing emissions, their fractional uptake, rather than just their absolute magnitude, would need to increase to maintain atmospheric CO_2 at present levels.
2. The broad mechanisms for historically-increasing uptake of CO_2 by land and ocean are well established to be driven by the increasing atmospheric CO_2 — as the DOE report authors in fact note in the 3rd full paragraph on p18. Crucially, therefore, these **natural sinks will only continue to increase if atmospheric CO2 itself continues to increase** (Arora et al., 2020; Global Carbon Project, 2024; Keenan and Williams, 2018; MacDougall et al., 2020). As a result, these sinks can never exceed emissions — and atmospheric CO_2 will continue to increase — until emissions are curtailed and atmospheric levels start to reduce. (The precise evolution of land and ocean carbon sinks after peak emissions is an area of widespread research (MacDougall et al., 2020; Sanderson et al., 2024).)
3. **The historical evolution of the land and ocean carbon sinks (which the report exclusively focuses on) is well known to be a poor predictor of future changes.** In the

ocean, the CO_2 uptake capacity decreases with every unit of carbon absorbed historically – a well-established consequence of ocean carbonate chemistry known as “buffering” (Bates et al., 2014). Future projections of the ocean carbon cycle disagree only about how rapidly this slowdown will progress, since it depends on the rates of ocean circulations and changes therein (Arora et al., 2020). Rising sea-surface temperatures also reduce the solubility of CO_2, directly impeding the ocean's uptake capacity (Masson-Delmotte et al., 2023), which in simulations of medium and high emissions scenarios impacts fractional ocean sink uptake (Arora et al., 2020). Future circulation changes such as a weakening Atlantic Meridional Overturning Circulation could also act to further decrease fractional ocean sinks (Schaumann and Alastrué de Asenjo, 2025), with some evidence of an observed weakening of sink strength in recent assessments of oceanic carbon trends (Müller et al., 2023). For the land, while elevated CO_2 can enhance plant growth, this effect is significantly constrained by nutrient limitations in the soil (Fleischer and Terrer, 2022) and negative effects of temperature on plant growth (Fernández-Martínez et al., 2019). Moreover, suppressed land carbon uptake was linked to the record atmospheric CO_2 growth in 2023-2024 (Global Carbon Project, 2024; Gui et al., 2024). This growth is being compounded by the thawing of carbon-rich permafrost, now considered a definite amplifying feedback to global warming (Canadell et al., 2023; Smith et al., 2022). Although the global forest sink has appeared stable in absolute terms, there have been regional shifts in uptake and much of this benefit has been offset by deforestation (Pan et al., 2024). In short, established biogeochemical principles and emerging observations for both land and ocean sinks point toward a future of progressively diminishing efficiency in absorbing anthropogenic CO_2, and an increase in airborne fraction as climate change increases (Canadell et al., 2023; Crisp et al., 2022).

In summary, the DOE report critically misrepresents the scientific understanding of land and ocean carbon sinks, using them to foster a misleading sense of complacency regarding human-induced climate change. It conflates the absolute magnitude of carbon uptake with the crucial fractional uptake, thereby ignoring the fact that natural sinks must increase their fractional uptake to mitigate rising emissions. Furthermore, the report largely omits extensive literature on the future behavior and potential degradation of these sinks. It fails to acknowledge that these natural sinks are primarily driven by increasing atmospheric CO2, and that their capacity to absorb carbon is not limitless, particularly for the ocean due to buffering and warming, and for land due to nutrient limitations, negative temperature effects, and permafrost thawing. By focusing exclusively on historical trends and neglecting the complexities and vulnerabilities of future sink behavior, the DOE report presents an incomplete and skewed picture that downplays the urgency of emission reductions.

Comment 5: On Urbanization and Temperature Records

Reference in CWG Report: Page 11 (Summary Statement); pp. 20-21 (Detailed Argument).

Comment Type: Technical. This addresses the report's inaccurate assessment of the Urban Heat Island (UHI) effect on global data.

Original Text/summary statement from CWG Report: "There is evidence that urbanization biases in the land warming record have not been completely removed from climate data sets".

The inclusion of data from urban and suburban weather stations does not appreciably change the large-scale averages in global temperature datasets like (Rohde et al., 2013). While the Urban Heat Island (UHI) effect is a real and well-documented local phenomenon, comprehensive studies have repeatedly shown that it has a minimal impact on the calculated warming trend for continents and the planet as a whole (Hausfather et al., 2013). This conclusion is supported by two primary lines of evidence. First, the standard scientific practice of **homogenization** effectively detects and removes non-climatic biases from the raw data, including the artificial warming signals from urbanizing stations (Hausfather et al., 2013). This finding holds even when only rural stations are used for breakpoint detection and correction in the homogenization process to avoid any risk of aliasing in a UHI signal (Ibid).

Second, results from these adjusted datasets are independently validated by **rural-only networks** (like the U.S. Climate Reference Network), which show nearly identical rates of warming (e.g., (Hausfather et al., 2016; Vose et al., 2014). This finding is reflected by the IPCC Regional fact sheet on Urban Areas, which states that urban heat effects have “a negligible impact on global annual mean surface air warming (very high confidence)" (Regional fact sheet - Urban Areas, 2025). Additionally, data from AIRS satellite measurements (Susskind et al., 2019) show comparable rates of warming as the full land station network during the period of overlap, suggesting minimal UHI biases in recent decades.

Modern reanalysis products like ERA5 and JRA-3Q also provide a constraint on the potential magnitude of UHI biases given the large amount of additional data they assimilate from satellites, planes, radiosondes. If urban warming were being incorrectly spread across large areas by surface station-based land temperature reconstructions, using a much larger volume of more spatially complete data would tend to catch this given resulting inconsistencies between variables over space and time (e.g. temperature and pressure). However, reanalysis products largely agree with surface temperature records in terms of global and regional land surface temperature trends.

Finally, the DOE report fails to place its discussion of potential UHI biases in the broader global context. This spatial structure of warming shows coherent warming patterns not confined to urbanized areas, with the strongest warming in remote high-latitude regions – broadly in-line with modeled expectations of warming patterns (Hahn et al., 2021). The world is also >70% oceans, so even the presence of a significant (>10%) bias in land temperatures, if it existed, would have a much smaller effect on resulting global surface temperatures.

Recommendations for revision

Based on a critical review of Section 3, "Human Influences on the Climate," of the DOE report "A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate," we recommend the following revisions to correct factual errors, address selective evidence presentation, and rectify issues with reference interpretation:

1. Correction of Misrepresentation of IPCC Scenarios

- *Revise the portrayal of IPCC scenarios:* The report should accurately reflect that IPCC scenarios are not predictions but exploratory "what-if" tools designed for risk assessment under deep uncertainty, as consistently stated by the IPCC.
- *Acknowledge the evolving nature of high-end scenarios:* Clearly state that the assessment of upper plausible bounds of future emissions evolves with changing technological, economic, and geopolitical factors, and that high-end scenarios remain necessary for exploring upper-bounds on risks and tipping points.
- *Correct the claim about overstated emissions projections:* The report should acknowledge that observed CO2 emissions, rather than solely tracking the low end of projections, are well-centered within the CMIP6 ScenarioMIP distribution.

2. Accurate Representation of Human Influence on Climate

- *Clarify the dominant role of human activities:* The report must unequivocally state that human activities, particularly greenhouse gas emissions, are not merely an additive factor to natural variability, but the primary and dominant driver of observed global warming since the industrial era. This statement aligns with IPCC AR6 and NCA5 conclusions.
- *Incorporate comprehensive attribution evidence:* Include the robust, multi-faceted evidence for human-caused warming, such as energy budget analyses, climate model simulations, and physical "fingerprints" of warming (e.g., cooling stratosphere, carbon isotopic ratios), which consistently show natural drivers having a negligible or even slightly cooling effect over the long term.

3. Comprehensive Discussion of Carbon Cycle and Natural Sinks

- *Distinguish between absolute and fractional uptake of carbon sinks:* The report should emphasize that for climate change, it is the *fraction* of human emissions taken up by land and ocean sinks that is crucial, not just their absolute magnitude - given that it is the fraction of future emissions which remain resident in the atmosphere which is the relevant measure of the degree to which sinks will counter emissions. The annual increase in CO_2 concentrations is about half of the CO_2 emitted because land and ocean processes currently absorb “excess” CO_2 at a rate approximately 50 percent of human emissions, but there remain risks that these sinks could weaken in the future.
- *Address the future behavior and limitations of natural sinks:* Include the extensive literature on the potential degradation and limitations of land and ocean carbon sinks due to factors like ocean buffering and warming, nutrient limitations in land ecosystems, negative temperature effects on plant growth, and permafrost thawing. The report's exclusive focus on historical trends is incomplete and misleading.
- *Correct the downplaying of human emissions:* Remove the misleading comparison of human emissions to natural exchanges that states human emissions are "only about 5 percent of the annual exchange with the atmosphere." Instead, clarify that human emissions disrupt the natural balance of the carbon cycle, leading to the long-term increase in atmospheric CO_2 concentrations.

- *Provide accurate context for ocean biological processes:* While the uncertainty of ocean biological CO2 uptake is noted, the report should clarify that, unlike on land, there is no clear mechanistic reason to expect an increase in ocean biology due to CO2 fertilization.

4. Corrected Assessment of Urban Heat Island (UHI) Effect

- *Acknowledge minimal UHI impact on global warming trends:* The report should reflect the scientific consensus that while the UHI effect is a local phenomenon, it has a minimal impact on large-scale global temperature averages and calculated warming trends.
- *Highlight effective data homogenization and rural-only validation:* Emphasize that standard homogenization practices effectively remove non-climatic biases, including UHI signals, and that results are validated by rural-only networks and satellite measurements showing consistent warming rates.
- *Consider the global context of warming:* Reinforce that warming patterns are coherent globally, not confined to urbanized areas, with strongest warming in remote high-latitude regions, and that oceans cover over 70% of the Earth's surface, further minimizing any potential land-based UHI bias on global temperatures.

5. Inclusion of a More Complete and Balanced Literature Review

- *Avoid cherry-picking non-consensus views:* The report should avoid selectively highlighting studies that present non-consensus views (e.g., on solar influence or UHI bias) as representative of scientific debate when the overwhelming weight of independent lines of evidence point to a clear consensus.
- *Present a balanced view of critiques and responses:* When presenting critiques of scientific findings, the report should also include the responses and counter-arguments from the broader scientific community, such as the critiques of statistical methods used in certain studies (e.g., (Richardson and Benestad, 2022) regarding solar influence).

6. Corrected Presentation and Contextualization of Atmospheric CO2 Concentrations

We suggest a revision of the CWG figure 3.1.2 as follows:

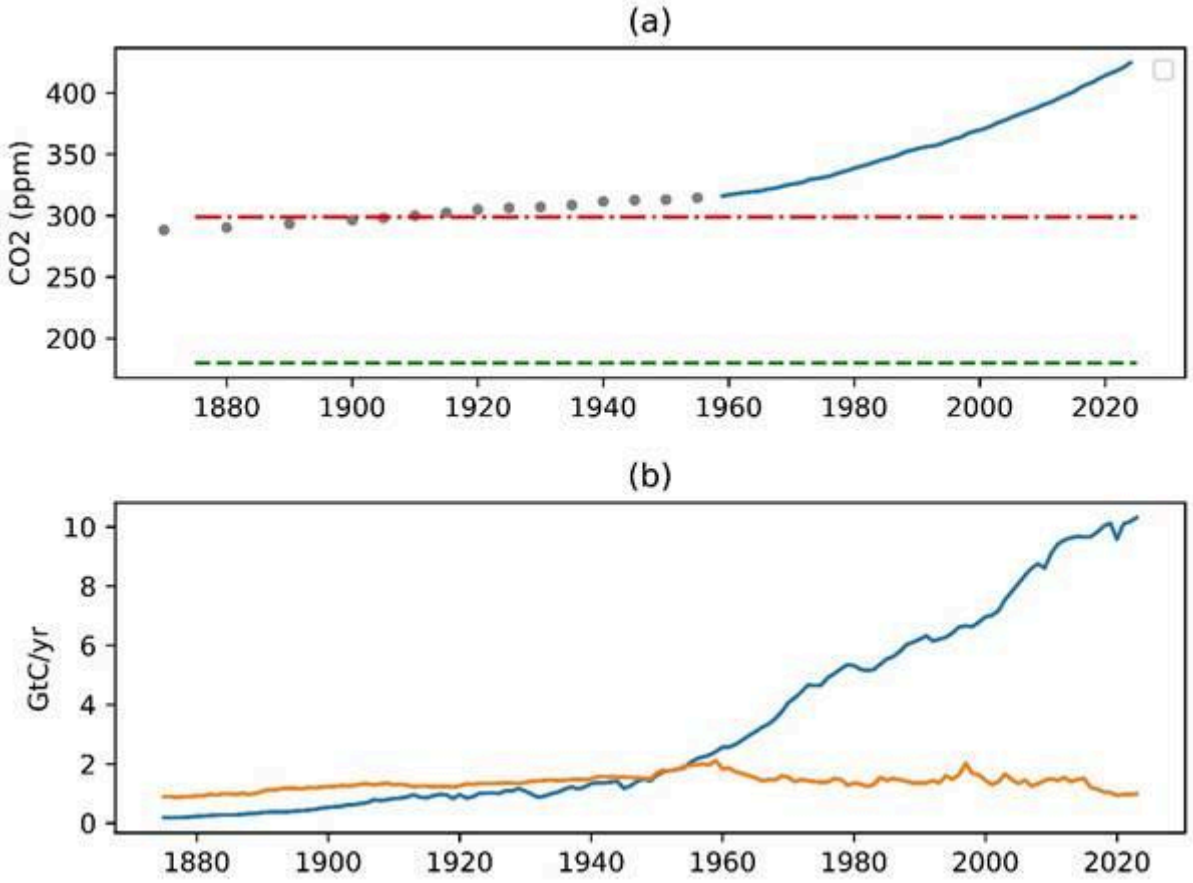

Figure 3.1.2 (suggested revision). (a) Yearly Average CO2 concentrations as measured at Mauna Loa (1959-2024; blue line)(Trends in CO2 - NOAA Global Monitoring Laboratory, 2025), and as reported by the IPCC (1870-1955; gray dots)(Anon, 2023). The green dashed line is the minimum CO2 level during recent glaciations (180 ppm). The red dot-dashed line is the maximum interglacial level (299 ppm). (b) Annual carbon emissions from fossil emissions excluding carbonation (blue line) and from land-use change (orange line), as reported by the (Global Carbon Project, 2024). All values are in billions of tons of carbon per year (GtC/Yr); multiply by 3.664 for values in billions of tons of CO2.

- **Address misleading visual representation of CO2 data:** Revise Figure 3.1.3 in the DOE report to:
    - Include earlier CO2 data (e.g., from the IPCC) to illustrate the dramatic change in growth rate beginning in the 1950s.
    - Adjust the y-axis scale to appropriately display the CO2 concentration data, avoiding the visual impact of making the curve appear shallower.
- **Remove irrelevant C3 and C4 plant thresholds:** Delete the discussion of C3 and C4 plant thresholds as they are irrelevant to the current and projected atmospheric CO2 concentrations.

- **Incorporate historical CO2 context:** Plot the interglacial maximum CO2 concentration alongside the glacial minimum to clearly show that current concentrations are far outside the range seen in the past 805,000 years and that the increase in the past 7 decades is comparable to the increase between glacial maxima and minima.
- **Clarify the shift in dominant emission sources:** Note that fossil fuel emissions became the overwhelmingly dominant source of CO2 additions to the atmosphere from the 1950s onward, exceeding land-use change emissions.

Suggested update to summary statements

Given the above we suggest the following update to the Chapter 3 summary statements:

- **On the Cause of Recent Warming:** Human activities are the dominant cause of the observed global warming since the industrial era. While the climate is naturally variable on all timescales, this variability has had a negligible contribution to the long-term warming trend compared to the influence of anthropogenic greenhouse gas emissions.
- **On the Role of Natural Influences:** Major scientific assessments, including the IPCC AR6 and NCA5, conclude that the contribution of solar activity to recent global warming is negligible. While a wide range of literature is assessed, the overwhelming evidence from multiple independent lines of analysis indicates that natural drivers, including the sun, cannot account for the observed warming.
- **On the Use of Climate Scenarios:** IPCC climate scenarios are not predictions but are exploratory "what-if" tools designed for risk assessment under deep uncertainty. Observed global CO_2 emissions are currently tracking within the middle of the range of the most recent CMIP6 scenarios. While the use of the extreme scenario RCP8.5 as a 'business-as-usual' projection has been characterized as misleading, high-end scenarios remain necessary tools for understanding potential high-risk climate outcomes and tipping points.
- **On the Carbon Cycle:** Land and ocean carbon sinks have increased their absolute uptake of CO2 in response to rising anthropogenic emissions, absorbing a relatively constant fraction of about 50% of emissions to date. However, this historical trend is not a reliable predictor of future behavior, as the capacity of these sinks is projected to be limited by factors such as ocean carbonate chemistry (buffering), warming ocean waters, nutrient constraints and warming effects on land carbon uptake. Focusing only on increasing absolute uptake fosters a misleading sense of security.
- **On Urbanization and Temperature Records:** The scientific consensus, reflected in IPCC reports, is that the Urban Heat Island (UHI) effect has a minimal to negligible impact on calculated global and continental-scale warming trends. Standard data homogenization procedures effectively detect and remove non-climatic biases from the temperature record, and these corrected datasets are independently validated by rural-only networks and satellite measurements.

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Section 3.1.1: Historical Radiative Forcing

Dr. Theodosios Chatzistergos and Dr. Ted Amdur

1. Suggestion of bias by IPCC by stating that it prefers TSI series that imply minimal solar change since preindustrial times

2. Suggestion that existing plausible TSI reconstructions cover the range of no to most of 20th century warming being attributable to the Sun.

3. Exaggeration of ACRIM-gap issue

To correct these technical flaws, the following changes must be made in the DOE CWG report document:

1) Update the text to correctly cite the relevant scientific literature and make sure to mention that it is predominantly showing that solar influence on Earth's climate has a rather small net effect between Maunder minimum and present/second half of 20th century.
2) Remove erroneous statements about the IPCC reports.
3) Either remove the discussion about ACRIM-gap or present it in an unbiased way, highlighting that current understanding through various independent studies disfavors the hypothesised TSI increase during the ACRIM-gap.

1) IPCC reasoning for solar forcing datasets

Page: 12
Comment type: Technical
Comment: the DOE report asserts that IPCC chose the TSI forcing datasets based on their preference for series that exhibit small variations. This is erroneous and misleading. The selection of total solar irradiance (TSI) reconstructions for use in climate modeling, such as those adopted in CMIP6 and the IPCC's Sixth Assessment Report (AR6), is not based on the magnitude of variability they imply, but rather on the scientific robustness and performance of the models themselves (Matthes et al. 2017). For CMIP6, the two TSI reconstructions used were **SATIRE** (Yeo et al. 2014, Wu et al. 2018) and **NRLTSI** (now referred to as NNLTSI) (Coddington et al. 2016). These were chosen because they represent the most advanced models available at the time, SATIRE being a semi-empirical physics-based model, while NRLTSI a proxy (regression) model. Both models show excellent agreement with direct satellite observations of TSI and have undergone extensive scrutiny and validation (Coddington et al. 2019; Chatzistergos et al. 2025; Wu et al. 2018).

2) Current state in long-term TSI reconstructions

Pages: 12-13
Comment type: Technical
Comment: This DoE relies on a publication by Connolly et al. 2021 to argue for a huge uncertainty in the secular trend in TSI reconstructions, thus suggesting that depending on the choice of TSI series one can attribute no or most of the 20th century warming. Although there is indeed uncertainty in the secular trend of irradiance variations, Connolly et al. 2021 and as a consequence the DoE report exaggerate this by cherry picking outdated and implausible TSI series and presenting them as equally probable to others.

To do this, the DoE report ignores the vast majority of scientific literature on the topic of solar variability and irradiance measurements and modeling. All recent and comprehensive review papers about solar irradiance modelling are ignored (e.g. Solanki et al. 2013; Chatzistergos et al. 2023).

In particular, Chatzistergos et al. 2023, collected most (if not all) available reconstructions of total solar irradiance (TSI) that cover the period 1700-1986 grouping different versions of individual models together. They found 24 groups of models, out of which 21 suggest a rather small secular trend (here defined as difference between activity minima in 1986 and 1700) that is between −0.09 and 2.2 Wm^{-2}. Only 3 groups of models were found to exhibit a high secular trend (Hoyt and Schatten 1993; Egorova et al. 2018; Fligge and Solanki 2000). As noted in Chatzistergos et al. 2023, the study by Fligge and Solanki 2000 is “considered replaced by the newer SATIRE-family models”.

The model by Hoyt and Schatten 1993 has been thoroughly discredited (Chatzistergos 2024). It introduced arbitrary adjustments and even fabricated data, some of which appear to have been copied from another solar index. The Egorova et al. 2018 model relies heavily on assumptions about the Sun’s quietest possible state, a critical and uncertain input. This approach has been strongly criticized by Yeo et al. (2020), who demonstrated that the long-term TSI variability in the Egorova et al. 2018 model is exaggerated. The Yeo et al. 2020 study established a robust constraint on the dimmest state of the Sun, the lowest expected value in TSI, to be **2 ± 0.7 W/m²** below the value of the 2019 minimum, thus excluding the Egorova et al. 2018 reconstruction. Overall, despite the uncertainty in estimating the secular trend in TSI variations, current models suggest a relatively weak secular trend, and all high variability models have been criticized or even rendered invalid (in the case of Hoyt and Schatten 1993). While old versions of models that were at some point suggesting high secular trends, have been typically revised and now exhibit low secular trends.

Chatzistergos et al. 2023 also states: “often older versions are used in the literature ignoring the fact that newer versions, overriding the earlier ones, are available. We caution against using outdated reconstructions.”

This comment addresses the main issue with the presentation of TSI reconstructions by Connolly et al. 2021. In particular, Connolly et al. 2021 cherry-picked a set of 16 TSI series, deliberately balancing the sample to include eight with low long-term variability and eight with high variability. To achieve this equal division, they included outdated or superseded versions of

some models, while they ignored many low variability TSI reconstructions. Connolly et al. 2021 did not clarify how the division between high and low variability series was done, but all high variability series they used lie outside the constraint imposed by Yeo et al. (2020), so it is safe to consider them here as TSI series that exhibit variations exceeding **2 ± 0.7 W/m²** below the value of the 2019 minimum.

The high variability group in Connolly et al. 2021 includes:

- **Hoyt and Schatten (1993)** has been thoroughly discredited (Chatzistergos 2024). It introduced arbitrary adjustments and even fabricated data, some of which appear to have been copied from another solar index.
- **Lean et al. (1995)** is an early and now outdated version of the NRL model. It has long been superseded by newer versions, latest by NNL.
- **Bard et al. (2000)** reconstructed TSI by applying a simple linear scaling to the production rates of cosmogenic isotopes (e.g., ^{10}Be), without accounting for nonlinear effects such as geomagnetic modulation (Usoskin 2023). More recent and physically realistic models, such as **SATIRE-M** (Wu et al. 2018), explicitly incorporate this and indicate substantially lower long-term TSI variability. Furthermore, Delaygue and Bard (2011) revised the original Bard et al. (2000) reconstruction downward, but Connolly et al. (2021) ignored this update.
- **Shapiro et al. (2011)** an older version of Egorova et al. 2018
- **Egorova et al. (2018)** Connolly et al. 2021 included this model four times (five considering the Shapiro et al. 2011 too) as distinct reconstructions. This model is criticized by (Yeo et al. 2020)

An additional important point is that in all these eight high variability TSI series used by Connolly et al. (2021), the long-term TSI variability was not derived from observational constraints but was instead imposed as a model assumption. Modern updates of the NRL model (Coddington et al. 2019; Lean 2018) and Hoyt and Schatten 1993 (Chatzistergos 2024), have shown that high the variability that was imposed in the earlier versions is inconsistent with direct TSI measurements and have therefore been revised to show lower variability.

Thus, it is misleading based on the above to consider these high variability TSI reconstructions equally probable as the low variability ones. Furthermore, Connolly et al. (2023), although not cited in the DoE report, extended the analysis by Connolly et al. (2021) to include 27 TSI series, but the same criticism stands. They again used multiple outdated and overwritten versions of models, while the reconstructions by Hoyt and Schatten (1993) and Bard et al. (2000) are the ones resulting in their highest attribution of temperature change to the Sun, both TSI series discontinued and shown to have methodological issues. Richardson and Benestad (2022) has also criticized the methodology used by Connolly et al. (2021) to attribute temperature changes to TSI.

Overall, while uncertainty remains regarding the precise magnitude of long-term solar irradiance trends, Connolly et al. 2021 significantly overstates the Sun's role in recent climate change by

selectively including outdated, superseded, and implausible TSI reconstructions that are inconsistent with modern observational and theoretical understanding.

3) ACRIM-gap

Page: 13
Comment type: Technical
Comment: The DoE report exaggerates the significance of the ACRIM-gap. The delay in the launch of ACRIM2 led to the necessity of using data from Nimbus7-ERB and ERBE-ERB. Nimbus7-ERB is the oldest space-based TSI measuring instrument we have, while ERBE-ERB has lower quality and took observations more sporadically than Nimbus7-ERB. The whole ACRIM-gap issue originates in unaccounted sensitivity changes in either or both of those missions. Nimbus7-ERB suggested a slight increase in TSI between 1990-1992, while ERBE-ERB did not.

While a subset of literature has argued for decades that, through a certain interpretation of the ACRIM-gap, solar irradiance changes since 1978 are unknown or upwards, we have independently led two scientific studies that demonstrate a common understanding of the trends in solar irradiance since 1978, regardless of one's chosen interpretation of the ACRIM-gap.

First, Amdur and Huybers (2023), followed by Amdur and Huybers (2025), developed an independent statistical analysis using a sophisticated Bayesian approach inspired by the time series econometrics literature. In these studies, with clearly defined priors that make no *a priori* assumptions regarding the relative merits of using a given satellite or solar proxy in filling the ACRIM-gap, they find that the best estimate of Total Solar Irradiance (TSI) using almost all available observational satellite data supports a downward trend in TSI since 1978, in line with most reconstructions of irradiance.

Second, Chatzistergos et al. (2025) performed irradiance reconstruction with the physics-based semi-empirical model SATIRE-S using datasets that span the entire ACRIM-gap period and extend beyond it. This modeling approach returns long-term TSI trends that are independent of direct TSI measurements, although some uncertainty remains due to the quality of the input data. By applying the model separately to two independent datasets, Mt. Wilson magnetograms and reconstructed unsigned magnetograms from Meudon Ca II K observations, Chatzistergos et al. 2025 demonstrated that both reconstructions contradict the TSI increase suggested by some during the ACRIM-gap, providing independent evidence against that upward trend.

Thus, while the issues leading to the ACRIM-gap contribute to the uncertainty in the long-term trends in direct TSI measurements, both statistical and physics-based reconstructions relying upon independent data find a common resolution to the ACRIM-gap, and it is important to note that these findings are consistent with existing TSI composites that do not exhibit substantial disagreement (Kopp 2025). In particular, Chatzistergos et al. (2023) in their Table 1 lists the trend between activity minima of cycle 25 (2019) and 22 (1986) and shows that it is rather small

ranging between -7.6 mW m^{-2} y^{-1} (Montillet et al. 2022) and 1.8 mW m^{-2} y^{-1} (Scafetta 2023) (the latter is an informal update of ACRIM TSI composite). Most recently, Amdur and Huybers (2025) found a trend of -5 mW m^{-2} y^{-1} over this period.

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Section 3.2.1: Emissions scenarios

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Reference in CWG Report: pp. 15

Comment Type: Technical. This addresses the report's misinterpretation of the use of climate scenarios.

Original Text/summary statement from CWG Report: "Although the IPCC does not claim its emission scenarios are forecasts, they are often treated as such".

Comment:
The text suggests that the IPCC is unclear about the meaning of emission scenarios, which is not the case. Each Working Group report in AR6 explicitly states how emission scenarios are to be interpreted and used. For example, here is the statement of the emissions scenarios in BOX SPM .1of AR6 Synthesis Report (IPCC, 2023): “Modelled scenarios and pathways are used to explore future emissions, climate change, related impacts and risks, and possible mitigation and adaptation strategies and are based on a range of assumptions, including socio-economic variables and mitigation options. These are quantitative projections and are neither predictions nor forecasts. IPCC is neutral with regard to the assumptions underlying the scenarios in the literature assessed in this report, which do not cover all possible futures”.

Reference in CWG Report: pp. 16

Comment Type: Technical. This addresses the report's misinterpretation of the development of climate scenarios.

Original Text/summary statement from CWG Report: "The IPCC developed a new set of scenarios for AR6, the “Shared Socioeconomic Pathway” (SSP) scenarios, which have continued the bias shown in the RCP and SRES scenarios".

Comment:
This text is false because the SSP scenarios were developed not by the IPCC but by international science communities. It is important to recognize that the AR6 WGIII (Riahi et al., 2022) assessed a large number of global modelled emissions pathways (about 1202) which were categorised based on their assessed global warming over the 21st century; categories range from pathways that limit warming to 1.5°C with more than 50% likelihood (noted >50% in this report) with no or

limited overshoot (C1) to pathways that exceed 4°C (C8). IPCC AR6 WGI assessed the climate response to selected five illustrative scenarios based on Shared Socio-economic Pathways (SSPs) that cover the range of possible future development of anthropogenic drivers of climate change found in the literature. It is also very important to note that AR6 WGIII Chapter 3.3 comprehensively assessed feasibility of emissions scenarios including SSP5-8.5 and SSP1-1.9. Those assessments in IPCC AR6 reports are not recognized in this report.

IPCC, 2023: Summary for Policymakers. In: Climate Change 2023: Synthesis Report. Contribution of Working Groups I, II and III to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [Core Writing Team, H. Lee and J. Romero (eds.)]. IPCC, Geneva, Switzerland, pp. 1-34, doi: 10.59327/IPCC/AR6-9789291691647.001

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Review of the DOE document titled "A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate"

David Crisp

I am a former Senior Research Scientist at NASA's Jet Propulsion Laboratory. I led the science teams for NASA's Orbiting Carbon Observatory missions (OCO-2 and OCO-3), focusing on satellite-based greenhouse gas measurements.

I am the first author of Crisp et al. (2022). The DOE CWG report claims to rely heavily on this review paper to inform its understanding of land-ocean-atmosphere carbon cycling. Regarding this paper, the DOE CWG report states,

> *The relation between emissions and concentration is determined by the global carbon cycle of land and ocean processes that exchange carbon with the atmosphere. Our understanding of these processes was reviewed by Crisp et al. (2021) [sic]…. While land vegetation has been responding positively to more atmospheric CO_2, uptake of extra CO_2 by ocean biological processes remains too uncertain to be measured reliably. Our current understanding of these and many more carbon cycle processes was reviewed by Crisp et al. (2021) [sic].*

This is a comment on Section 3.2.2, which explains how the global carbon cycle links fossil fuel emissions to atmospheric CO_2 levels. It describes the major carbon reservoirs and the annual exchange of roughly 200 gigatonnes of carbon between the atmosphere, land, and oceans, noting that human activities add about 10 GtC/year — only ~5 % of that exchange. Historically, land vegetation and oceans have removed about half of these human emissions, with land uptake increasing more than ocean uptake, but model estimates of these fluxes vary widely, introducing uncertainty into projections of future CO_2 concentrations and climate change.

- **Page:** 17
- **Comment Type:** Reference
- **Comment:** 1st paragraph: Our work is cited as "Crisp et al. (2021)." The paper included in their reference list is actually the review paper by Crisp et al. (2022). There is no paper by Crisp et al. (2021) covering this topic to the best of our knowledge.
- **Fix:** Correct this reference.

- **Page:** 17
- **Comment Type:** Technical
- **Comment:** 2nd paragraph: The total mass of carbon in the atmosphere is increasing over time as the carbon dioxide concentration increases. Crisp et al. (2022) quoted a mass of 877

Peta grams of Carbon (Pg C; one Pg C is equivalent to billion tons of carbon (GtC) or 3.67 billion tons of CO_2), which was a good estimate in 2020, when the atmospheric CO_2 dry air mole fraction (a measurement of concentration) was around 412 ppm. Here, their estimate of "about 850 GtC", was relevant around a decade ago, when the average CO_2 concentration was closer to 400 ppm.

- **Fix:** Correct this error with a more recent value.

- **Page:** 17
- **Comment Type:** Technical
- **Comment:** 2nd paragraph: The quoted gross annual exchange of carbon between land and the atmosphere and between the ocean and the atmosphere are erroneous (and their relative values seemingly reversed). Here, they state that land exchanges 80 Gt C with the atmosphere and the oceans exchange 120 Gt C with the atmosphere. They do not cite specific sources for these numbers and they do not come from Crisp et al. (2022). In Crisp et al. (2022) the values provided for gross carbon flux are 120-175 GtC per year for land-atmosphere and 90 GtC for gross ocean-atmosphere.
- **Fix:** Correct these errors and provide a correct reference.

- **Page:** 17
- **Comment Type:** Technical
- **Comment:** They end this paragraph by noting that the 10.3 GtC emitted by fossil fuel use and cement manufacturing is only about 5% of annual emissions by the land biosphere and oceans. This is a half-truth that fails to recognize that the atmosphere and climate respond to the net emissions and removals of carbon, not the gross fluxes. The land biosphere and ocean both add and remove carbon from the atmosphere whereas fossil fuel use and other human activities only add carbon to the atmosphere. When averaged over the globe and over the year, the land biosphere and ocean remove as much carbon as they emit. The land and ocean also remove over half of the anthropogenic emissions, yielding net emissions that are negative and only about half as large as the positive anthropogenic emissions. In this context, the net anthropogenic emissions are more than twice as large as the net natural CO_2 fluxes (Friedlingstein et al, 2025).
- **Fix**: Change the wording to reflect the above facts, specifically that the net anthropogenic emissions are twice as large as the net natural CO_2 fluxes.

- **Page:** 18
- **Comment Type:** Technical
- **Comment:** 1st paragraph: Here, they recognize that existing observations show that natural processes in the land biosphere and ocean consistently remove about 50% of the

anthropogenic CO_2 emissions. They also note that this ratio has been preserved as the fossil fuel emissions have increased. They attribute the increasing land uptake to the "global greening" phenomena described in Section 2.1 of this work.

However, Section 2.1 fails to recognize that the increasing leaf area index (LAI) does not always indicate increased CO_2 uptake. In particular, as noted in Crisp et al. (2022), many regions of the tropics, where the largest increases in LAI have been seen over the past 50 years, have started to transition from net CO_2 sinks to net sources of CO_2. These changes are due to the impacts of human activities (e.g. deforestation, forest degradation) and climate change (increasing temperature, drought, vapor pressure deficit).

The authors also fail to note that while tropical land is absorbing less CO_2, these losses are being compensated mostly by increased carbon uptake by forests at mid and high latitudes. There, climate change is driving longer, warmer growing seasons, and permafrost melt is allowing trees to grow deeper, more massive root systems that sequester more carbon in the soil. The rapid growth, combined with higher temperatures and more frequent droughts at these latitudes, produce larger and more frequent wildfires, which release CO_2. In summary, while the fraction of the anthropogenic CO_2 that has been absorbed by the land biosphere has remained almost constant in response to increasing atmospheric CO_2 abundances, different parts of the world are responding differently to climate change (Byrne et al., 2020). The focus on "global greening" is a massive oversimplification of the system.

- **Fix:** The authors should note that while natural CO_2 sinks in the land biosphere and ocean have absorbed roughly 50% of anthropogenic emissions since the late 1950s, the nature, location and biophysical processes controlling the land biospheric sinks are now changing rapidly in response to human activities (e.g., deforestation, forest degradation) and climate change. These rapid changes compromise our ability to predict the efficiency of these critical sinks in the future as climate continues to evolve in response to anthropogenic greenhouse gas emissions (Liu et al., 2017, 2020; Poulter et al., 2014; Sitch et al., 2015).

- **Page:** 18
- **Comment Type:** Technical
- **Comment:** The subsections titled "*CO_2 uptake by land processes*" and "*CO_2 uptake by ocean processes*" focus on the carbon cycle models used by the Global Carbon Project. For both land and ocean, they note that this set of models predicts a range of estimates for the decadal uptake of CO_2 by the land and ocean carbon sinks. They use these results to foster doubt in the utility of these methods for predicting future change in the carbon cycle as it responds to climate change. This is misleading to the reader, because they fail to note that

all of the Dynamic Global Vegetation Models (DGVMs) included in this set include CO_2 fertilization – a mechanism that the authors of this document advocate as a key benefit of increasing atmospheric CO_2 amounts (Bastos et al., 2019).

In general, this entire sub-section illustrates a very poor understanding of existing observations and models of the global carbon cycle and their response to human activities and climate change.

Fix: Please add a sentence or sentences to explain to the reader that CO_2 fertilization is included in DVGMs, but does not necessarily enhance growth due to the other factors identified above.

- **Page:** 18, 19
- **Comment Type:** References
- **Comment:** Figures 3.2.3 and 3.2.4 cannot be found in Friedlingstein et al. 2024.
- **Fix**: Please be more clear about the source of these figures and data. For instance, if it is from a figure in Friedlingstein et al. 2024, please cite the specific figure. If the authors downloaded data themselves and made this plot, please give the link or location of the data from where it was downloaded.

Comments on Other Chapters

- **Page:** 3
- **Comment Type:** Technical
- **Comment:** Chapter 2, section 2.1 (and chapter 9) assert that CO_2 fertilization will increase plant growth and crop yields. The proposed benefits of CO_2 fertilization are not realized in the DGVMs described on pg 18 because CO_2 availability is only one of several mechanisms that control plant growth. As any farmer knows, plant growth is rarely limited by the abundance of the most abundant nutrient. It is usually limited by the abundance of the least abundant nutrient. While increased CO_2 can accelerate plant growth in carefully controlled laboratory conditions, this rarely happens in nature or in large-scale agriculture. In most natural settings, plant growth is usually limited by water, nitrogen, phosphorus, sunlight or temperature. DGVMs include all of those effects (Vitousek et al., 2010; Worden et al., 2021; Yuan et al., 2019). The range of outputs produced by the models reflects uncertainties in the relative roles of these processes and their potential evolution with climate change. This behavior should foster serious concern (doubt) about the potential benefits of CO_2 fertilization in a changing climate. There is no discussion of this here or in chapter 2.1 or in Chapter 9.
- **Fix:** The authors should clearly qualify their statements on CO_2 fertilization, recognizing that in the presence of increasing atmospheric temperature or limitations in water, sunlight,

nitrogen or phosphorus, increasing the atmospheric CO_2 concentration is more likely to impede than promote the growth of plants.

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3.3 Urbanization influence on temperature trends

John M. Wallace and Chan-Pang Ng

1. Introduction

The authors devote an entire section of the report to the urban heat island effect (UHI), which might seem to some readers like an esoteric technical issue. This section is not a distraction: along with Section 8.3, its purpose in the report is to cast doubt on observations documenting the rise of surface air temperature in response to the burning of fossil fuels.

The UHI is defined in the DOE report's glossary as "The tendency for inhabited areas to be warmer than their rural surroundings, due to the replacement of natural land and vegetation with roads, parking lots, buildings, and waste heat sources." None of these factors significantly warm the global atmosphere, but they conspire to create a shallow layer of relative warmth below the nighttime temperature inversion over cities and suburbs.

The authors devote this section to critiquing the various attempts to evaluate the influence of urbanization upon the trends in surface air temperature, concluding with the cautionary statement, "…while there is clearly warming in the land record, there is also evidence that it is biased upward by patterns of urbanization and that these biases have not been completely removed by the data processing algorithms used to produce climate data sets."

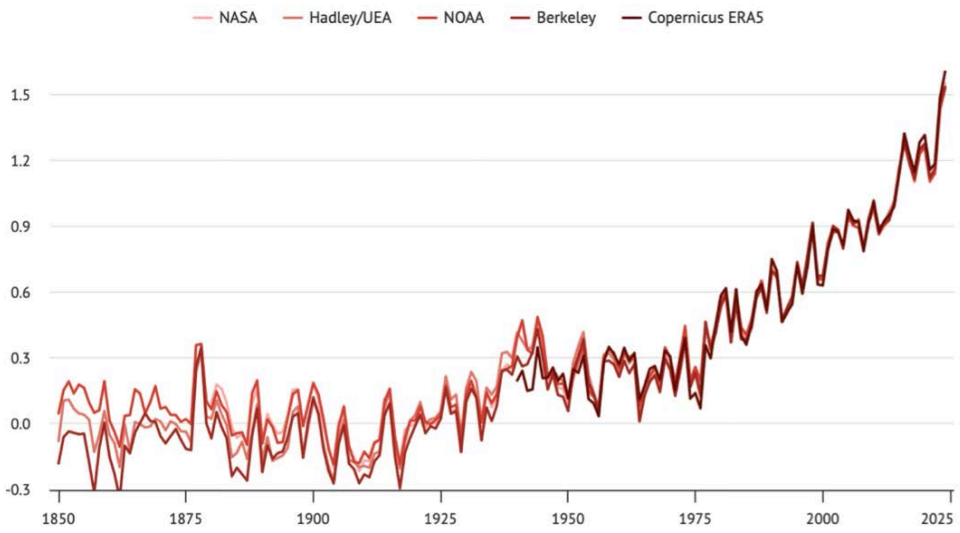

Figure 1. Annual mean, global mean surface air temperature as analyzed by the research groups listed in the legend. From the Berkeley Earth website. Temperatures are anomalies in °C relative to the 19th century.

Let's have a look at the data. Figure 1 shows extended time series of global mean surface air temperature computed by various climate centers, as indicated in the legend. In the 50-year

interval from the end of the hiatus in the mid-1970s to the present, global mean temperature has risen by almost 1°C.

Figure 2 shows the Berkeley Earth time series partitioned into the contributions from land and ocean grid points. During the 50-year interval 1975-2025, land temperatures have risen by ~1.5°C.

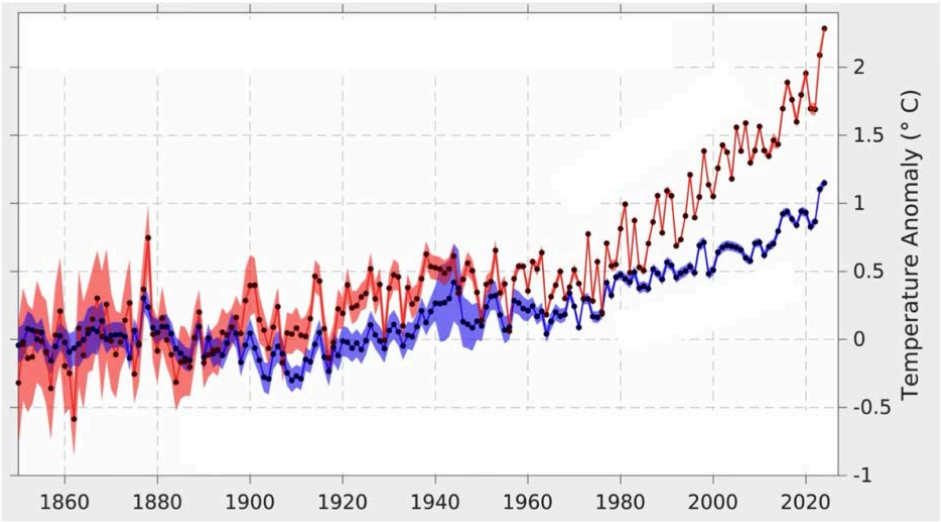

Figure 2. Time series of global mean surface air temperature. The red curve shows the average of land grid points, and the blue curve shows the average of ocean grid points. From the Berkeley Earth website. Temperature anomaly is once again relative to the late 19th century.

Figure 3 shows the geographical distribution of warm season (May through September)[1] surface air temperature trends expressed in °C per 50 years over the continental United States based on Berkeley Earth and ERA5. The warming is larger in the ERA5, but the large-scale patterns are similar in the two datasets, with warming throughout the country, strongest at the western and south-central states. The patterns based on the Hadley, GISS, and NOAA datasets (not shown) are similar.

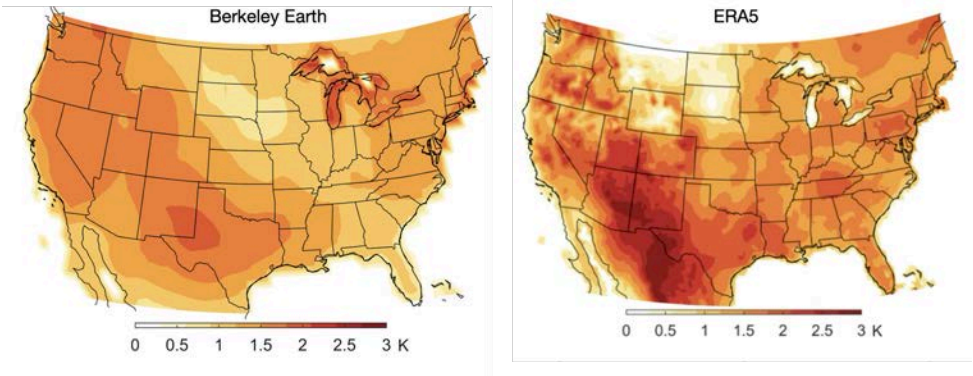

Figure 3 Warm season (May through September) surface air temperature trends based on Berkeley Earth and ERA5, in units of °C per 50 years.

[1] We show warm season means rather than annual means, because it is during the warm season that we experience the adverse impacts of extreme heat.

Averaged over the continental United States, the 50-year trend is somewhat larger in ERA5 than in the climate models. ERA5 exhibits more structure on scales of a few hundred km or less than the climate datasets because it's not subject to the spatial smoothing inherent in producing the observational datasets.

If the UHI were really producing a significant warm bias in the surface air temperature trends, one would expect the warming to be less prominent above the shallow nighttime temperature inversion. Figure 4 compares the pattern of 50-year temperature trend on the 123rd model level of ERA5 with the surface air temperate trend. This model level is about 500 m above the Earth's surface irrespective of the land elevation. That the two patterns are very similar indicates that the pattern of surface warming extends deep into the atmosphere.

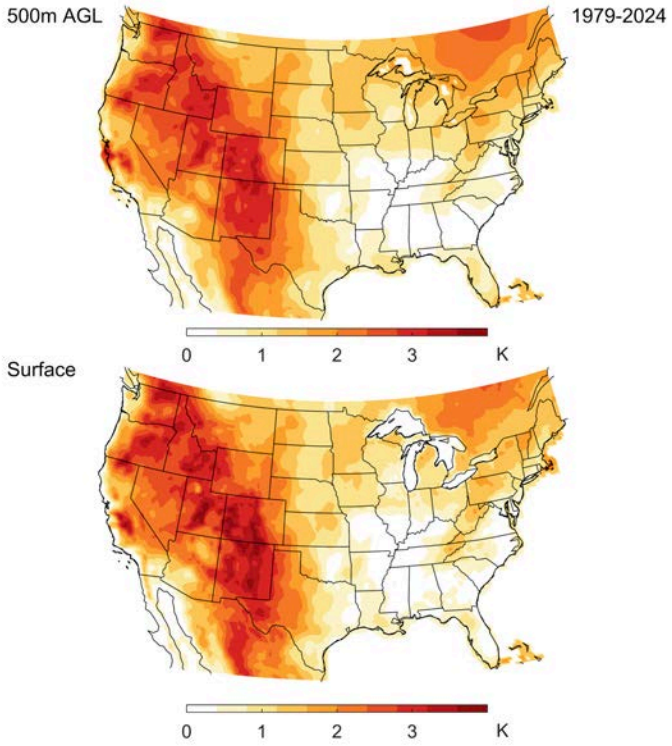

Figure 4. Annual mean temperature trends (°C/50 years), 1975-2024 in the ERA5 reanalysis: 2 m above the Earth's surface and on the 123rd model level, which is about 500 m above ground level irrespective of surface elevation.

The pattern of temperature trends in Figs. 3 and 4 bear no obvious relation to the pattern of population density over the continental United States, shown in Fig. 5.

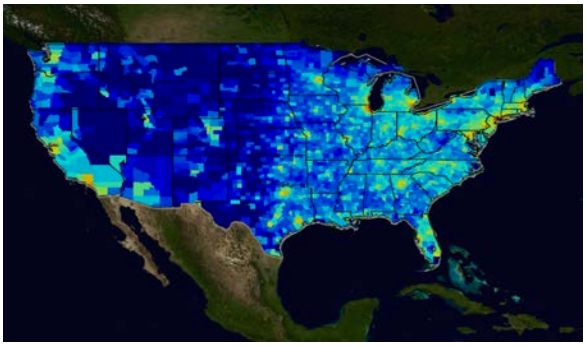

Figure 5. Population density over the continental United States based on the 2001 census. Provided by NASA's Scientific Visualization Studio.

Magnitudes of the 100, 75, 50, and 25-year trends in surface air temperature averaged over various domains and seasons based on Berkeley Earth are summarized in Table 1. The bolded values are consistent with the trends shown in Fig. 1. The trends are accelerating, as evidenced by the monotonic increase in the scaled values in all rows from the first to the fourth columns. The trends over the US exhibit little seasonality, except from 1999 onward, when the warm season trends have been larger than the cold season trends.

Table 1. Surface air temperature trends based on least squares best fits of Berkeley Earth data scaled in units of °C per 50 years.

	1924-2024	1949-2024	1974-2024	1999-2024
Global mean (year-round)	0.56	0.78	**1.03**	1.26
Global land (year-round)	0.74	1.09	**1.50**	1.63
Continental US (year-round)	0.59	0.89	1.39	1.30
Continental US (May-Sept)	0.59	0.90	1.42	1.70

Conclusions

We have shown that the magnitude of warm season temperature trends over the continental US since the mid 20th century are roughly comparable that of the global trends in land temperature (compare the values in Table 1). Since 1974, warming has prevailed over the entire country, strongest in the arid and semiarid regions of the southwest and weakest in the northern Great Plains (Figs. 3 and 4). The warming extends deeper into the atmosphere than the nighttime inversion (Fig. 4). It bears no obvious relation to the geographical distribution of population density (Fig. 5).

The evidence presented here does not support the contention in Section 3.3 of the DOE Report that the temperature trends in the observational datasets exhibit a substantial positive bias because of the urban heat island effect. We accordingly reject this attempt to cast doubt upon on the reliability of observations of temperature trends and we recommend that Section 3.3 be removed from the report.

Assessment of Chapter 4 "Climate Sensitivity To CO_2 Forcing"

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Executive Summary

This assessment critically reviews Chapter 4 of the DOE report, "Climate Sensitivity To CO_2 Forcing," identifying key misrepresentations of climate science:

1. **False Dichotomy:** The report wrongly presents climate science as a conflict between "models" and "data," ignoring the consensus approach of synthesizing multiple independent lines of evidence (models, observations, paleoclimate).

2. **Cherry-Picking Evidence:** It selectively highlights evidence supporting a low Equilibrium Climate Sensitivity (ECS) while downplaying or omitting contradictory evidence, particularly regarding aerosol forcing and the pattern effect.

3. **Internal Incoherence**: The report is undermined by critical logical inconsistencies. It champions a climate sensitivity estimate that, by its own stated policy threshold, justifies the very emissions controls it argues against; it cites technical evidence on the “pattern effect”, whose logical implication is a constant — not decreasing — climate sensitivity; and it applies contradictory standards by dismissing complex climate models while accepting simpler ones that fit its narrative.

4. **Misleading Focus on ECS:** The chapter's near-exclusive focus on the highly uncertain ECS metric neglects more policy-relevant metrics like Transient Climate Response (TCR) and entirely omits the crucial Transient Climate Response to Cumulative Emissions (TCRE), which underpins carbon budgets.

5. **Mischaracterization of Model Assessment:** It dismisses climate models and their "tuning" as systemic flaws, failing to acknowledge that the IPCC, and the climate science community more broadly, comprehensively assess model ensembles alongside other evidence, accounting for biases.

6. **Ignoring Recent Warming Trends:** The report's arguments for lower sensitivity overlook recent record-high global temperatures, accelerating warming, and the planet's growing energy imbalance, which might imply underestimation of future warming.

Introduction

This section critically reviews **Section 4 ("Climate Sensitivity To CO_2 Forcing")** of the DOE report "A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate" (DOE CWG). It identifies factual errors, instances of selective evidence presentation (cherry-picking), and issues with reference interpretation. We frame our assessment in sections, structured thematically by the key misrepresentations highlighted in the chapter.

Chapter 4 contains two primary summary messages, arguing that (1) data-driven estimates of Equilibrium Climate Sensitivity (ECS) are more reliable than climate models and indicate lower sensitivity, and (2) the IPCC AR6 assessment improperly raised the lower bound of likely ECS.

Our analysis finds that the chapter fundamentally misrepresents the consensus on climate sensitivity by:

1. **Creating a False Dichotomy:** It incorrectly frames science as a conflict between "models" and "data." The robust consensus, as articulated in the IPCC AR6 and the Sherwood et al. (2020) assessment, is based on the synthesis of **multiple independent lines of evidence**, including general circulation models, historical observations, and paleoclimatology.
2. **Systematic Cherry-Picking and Omission:** The chapter selectively highlights evidence and critiques that support a low-ECS narrative while ignoring or downplaying the overwhelming body of contradictory evidence and the convergence of findings that led to the AR6 conclusions. It leverages selective citation of the literature to erroneously support the author's preferred assessment of low ECS, while incorrectly and misleadingly rejecting the potential for large influence of uncertain factors which might complicate the low assessment.
3. **Misleading Focus on ECS:** The chapter's near-exclusive focus on the highly uncertain, multi-century metric of ECS is a form of issue selection bias, while neglecting more policy-relevant metrics. Minimal attention is devoted to TCR (the transient rate of warming in response to gradually increasing CO2 concentrations). Further, the report completely fails to address the cornerstone policy metric which accurately relates cumulative carbon emissions to warming - the Transient Climate Response to Cumulative CO_2 Emissions (TCRE).

Further, the report fails to recognize that recent years have seen record high global mean temperatures, which highlight that the 1.5°C goal of the Paris agreement is almost certain to be exceeded, that prior assessed trends might have underestimated the forced warming trajectory and that significant planetary radiative imbalances may indicate additional committed warming. These

issues are all deeply uncertain, but are core topics of active research represented in the climate literature and which highlight a risk that prior assessments might have underestimated future warming potential. By failing to address these issues, the DOE CWG presents a narrative justifying complacency in the need for emissions reductions at a time when observed warming rates are a justifiable cause for extreme concern.

Furthermore, the report's central argument is logically incoherent even on its own terms. The authors frame the entire chapter around the policy relevance of Equilibrium Climate Sensitivity (ECS), stating that "no CO2 emission controls are economically justifiable for ECS below 2.0°C" **[page 25]**. They then explicitly endorse the work of Lewis as their preferred alternative to the IPCC consensus, citing his analysis which "found that climate sensitivity is estimated to be much lower and better constrained... – median 2.2°C (1.8–2.7°C in the 17–83 percent likely range...)" **[page 28]**. The report thus concludes by championing an ECS estimate whose median value and entire likely range fall squarely within the zone where, by their own stated threshold, emission controls are economically justified. This fundamental self-contradiction reveals that the report's conclusions do not logically follow from its own preferred evidence.

This comment documents these scientific inaccuracies and omissions to ensure the official record reflects the current state of scientific understanding as represented in major assessments like the IPCC AR6 and the U.S. Fifth National Climate Assessment (NCA5). We discuss these issues below, followed by recommendations for the revision of the DOE CWG.

Detailed assessment of report conclusions

Here we expand on the key elements where we find the conclusions and assessment of the DOE CWG for chapter 4 to be incomplete or misrepresentative of best available scientific knowledge.

Comment 1: A Mischaracterization of Assessment Methodology

Reference in CWG Report: Page 25 (Summary Statements); pp. 26-28 (Detailed Argument)

Comment Type: **Technical**. This addresses the report's misrepresentation of how scientific assessments of climate sensitivity are conducted.

Original Text/summary statement from CWG Report: "There is growing recognition that climate models are not fit for the purpose of determining the Equilibrium Climate Sensitivity (ECS) of the climate to increasing CO2. The IPCC has turned to data-driven approaches including historical data and paleoclimate reconstructions, but their reliability is diminished by data inadequacies."

Implementation: The critique will dismantle the **false dichotomy** between "models" and "data". It will explain that the consensus approach, as used by the IPCC, is a **synthesis of multiple independent lines of evidence** (models, observations, paleoclimate). The implementation will clarify that models are used as crucial tools to test physical theories and are not treated as simple calculators of ECS.

Comment:

The assertion that *"climate models are not fit for the purpose of determining...ECS" [summary statement from CWG, page 25]* fundamentally misunderstands how recent climate assessments use them. The IPCC AR6 does not treat climate models as simple calculators for ECS, where the distribution of model results is taken as the final answer. In fact, the DOE report itself acknowledges that the IPCC has actively moved away from this approach, noting that that *"Because of concerns about model tuning and the high sensitivity to cloud parameterizations, AR6 (2021) did not rely on climate model simulations in their assessment of climate sensitivity, relying instead on data-driven methods" [DOE CWG, page 27].* Models are instead used as one crucial line of evidence among several, including historical observations, paleoclimate data, and process understanding. Their primary role is to serve as laboratories for testing physical theories about climate feedback—such as those related to water vapor and clouds—which are essential components of the overall sensitivity assessment. Therefore, the chapter critiques a simplistic methodology that the scientific community itself does not use.

> "Data-driven ECS estimates tend to be lower than climate model-generated values. The IPCC AR6 upper bound for the likely range of ECS is 4.0°C, lower than the AR5 value of 4.5°C. This lowering of the upper bound seems well justified by paleoclimatic data. The AR6 lower bound for the likely range of ECS is 2.5°C, substantially higher than the AR5 value of 1.5°C. This raising of the lower bound is less justified; evidence since AR6 finds the lower bound of the *likely* range to be around 1.8°C. " [DOE CWG, page 25]

This framing is a mischaracterization of the climate science which informed AR6. The central advance of the IPCC AR6 assessment of ECS, heavily informed by a broad independent assessment process (Sherwood et al., 2020), was not a shift away from models and to historical data, as the chapter implies. Rather, it was the formal synthesis of three complementary lines of evidence (Forster et al. 2021; Sherwood et al. 2020):

1. *Process Understanding:* Knowledge of climate feedbacks (e.g., water vapor, clouds, albedo), much of which is supported by theory and observational evidence and tested in climate models
2. *Historical Evidence:* The observed warming record, combined with estimates of forcings and ocean heat uptake.
3. *Paleoclimate Evidence:* Reconstructions of past climates, such as the Last Glacial Maximum and the mid-Pliocene, which provide constraints on how the Earth system has responded to past large-scale forcing changes .

Sherwood et al. (2020) demonstrated that these three lines of evidence combined provide a likely (66%) ECS range of 2.6 to 3.9°C, though different lines of evidence constrain different aspects of the distribution. As such, observed recent warming alone provides a strong lower bound, but only a weak upper bound on ECS (Sherwood et al. 2020; Armour et al. 2024). By presenting these as competing methods, with models being "unfit", the DOE chapter ignores the very source of confidence in the AR6 assessment. No single line of evidence is sufficient on its own; the strength of the conclusion lies in the collective information obtained by combining sources.

The report accepts that paleoclimate data justifies lowering the IPCC's upper bound, noting that the cooling during the last glacial maximum makes very high ECS values unlikely [see discussion of last glacial maximum, page 28 of the CWG]. However, it omits the other side of that coin: that same data makes very low ECS values equally unlikely, as a low sensitivity would be insufficient to explain either the dramatic cooling of the ice age, or observed warming over the last century, as summarised by Sherwood et al. (2020): "An S [ECS] value lower than 2 K is difficult to reconcile with any of the three lines of evidence." Moreover, the comparison of past climate states to present and future climates is not a straightforward apples-to-apples comparison: necessary assumptions that must be made in order to compare the climate feedback processes in a cold climate to those in a warm climate introduce additional uncertainty, especially regarding the upper bound (Budyko, 1969; Caballero and Huber, 2013; Sellers, 1969; Shaffer et al., 2016; Sherwood et al., 2020)

The report's critique of climate models is based on a contradictory and inconsistent standard. It argues that large-scale General Circulation Models (GCMs) cannot be trusted because they do not explicitly resolve all the small-scale physical processes that determine climate feedbacks **[page 26]** . Yet, the report is perfectly willing to accept and promote conclusions from far simpler models — like the box energy balance model used in the Lee et al. study to analyze the pattern effect **[page 28]** (Lee et al. 2024). This inconsistent application of skepticism reveals that the report's standard for accepting model evidence is not based on scientific rigor, but on whether the model's conclusions align with its preferred narrative.

Comment 2: Selective Citation and Interpretation of Evidence

Reference in CWG Report: Page 25 (Summary Statement); p. 28 (Detailed Argument)

Comment Type: **Reference / Technical**. This targets the report's biased use of literature and downplaying of key uncertainties to support a low-ECS conclusion.

Original Text/summary statement from CWG Report: "The AR6 lower bound for the likely range of ECS is 2.5°C, substantially higher than the AR5 value of 1.5°C. This raising of the lower bound is less justified; evidence since AR6 finds the lower bound of the likely range to be around 1.8°C."

Implementation: The critique will show how the report **cherry-picks evidence**, primarily from a single author, to question the AR6 consensus. The critique will explain that this low-ECS narrative is built by selectively downplaying major uncertainties like the **"pattern effect"** and **aerosol forcing**, whose potential impacts could imply a higher ECS .

Comment:

Much of the critique of the IPCC-AR6 ECS assessment is based on a study from a single author, (Lewis, 2022) — which the DOE report uses to claim that Sherwood et al. (2020) is subject to "methodological errors, outdated input values, and use of subjective Bayesian priors in the analysis" [quote from page 28 of the CWG]. Lewis (2022) was addressed in Sherwood and Forest (2024), then further contested by Lewis (2025)

The primary issues in this debate can be categorised into 3 topics:

- The implementation of prior assumptions in Bayesian assessments of ECS.
- The role of 'pattern effect' on historical sea surface temperature (SST).
- The uncertainty range for historical aerosol forcing

We address each of these issues in turn, given their outsized role in the conclusions of the report and in recent literature. In both cases, the authors of the DOE CWG report make a categorical error in which they leverage selective citation of the literature to assert *high confidence* in there being a *low impact* of these processes on the historical record. In effect, they attempt to argue that since we don't know the magnitude of the pattern effect or historical aerosol cooling, it must be zero or near-zero (in the case of the pattern effect) or much less than typically presumed (in the case of aerosol cooling). Instead, an appropriate assessment of the scientific literature would conclude substantial uncertainty that is worthy of ongoing study considering its potentially significant impact.

The choice of priors in Bayesian assessment of ECS

In Bayesian assessments of Equilibrium Climate Sensitivity (ECS), the *prior* distribution represents the initial state of knowledge before new evidence is considered; this prior is then updated by the data (via a likelihood function) to produce a final *posterior* probability distribution. Sherwood et al. (2020) (S20) employed a subjective Bayesian framework, which seeks to synthesize all available knowledge — including process understanding, historical data, and expert judgment — into a single, coherent probabilistic statement. The strength of this approach is its transparency, as it forces all assumptions to be made explicit and quantifiable.

The subsequent debate, primarily between Nicholas Lewis (Lewis, 2022, 2025) and some of the S20 authors (Sherwood and Forest, 2024), illuminates a fundamental divide in statistical philosophy. The S20 authors defend the subjective approach, arguing that expert judgment is an unavoidable and essential component of analyzing complex systems and that making these judgments explicit is a scientific virtue. S20 also did not bury this foundational assumption; their analysis included extensive robustness tests to demonstrate the sensitivity of their results to different priors, acknowledging the influential role of this choice.

In contrast, Lewis advocates for an objective Bayesian approach, whose purpose is to produce a *calibrated* statistical inference where the uncertainty ranges approximate the properties of a frequentist confidence interval. Lewis's central critique is that the subjective priors used in S20 are subject to "evidence duplication," where an expert's prior belief is rationally based on the same body of evidence used to construct the likelihood, effectively double-counting the data and leading

to an overconfident result. Instead, Lewis (2022) employed a mathematically derived, noninformative Jeffreys prior, designed to let the data dominate the final posterior distribution.

This ongoing debate makes it clear that there is no universally accepted "correct" prior for ECS estimation; the choice depends on the epistemological goal of the analysis — but it is clear that estimates are sensitive to these foundational, and partly subjective, choices — and that proper assessment of prior assumptions and their influence on assessed likely ranges for ECS should be an ongoing priority in future IPCC reports.. A promising path forward involves developing physically-constrained priors, such as those derived from emergent constraints (Renoult et al., 2020), which ground the analysis in observable processes and may help bridge the divide between purely subjective belief and abstract mathematical convention.

However, we find that the DOE CWG report fundamentally misrepresents the scientific debate over the use of priors in climate sensitivity estimation by selectively platforming Lewis' work to advance a low-ECS narrative. The report frames Lewis' (2022) critique of the S20 as a correction of "methodological errors... and use of subjective Bayesian priors," thereby positioning the S20 study (and the IPCC AR6 consensus) as scientifically unsound. This is a systematic oversimplification of a deep and ongoing debate between two distinct statistical philosophies.

The Jeffreys prior (Jeffreys, 1946) used by Lewis prioritizes producing a calibrated inference where the data drives the result. However, while mathematically elegant due to their invariance properties, Jeffreys priors are not always appropriate, particularly for complex, well-studied systems like the Earth's climate because they are derived solely from a model's mathematical structure, and can be notoriously difficult to compute for high-dimensional models (Simpson et al., 2017). In contrast, the expert prior used in the Bayesian approach of S20 aims to transparently synthesize all available knowledge including expert judgment through a well established process known as prior elicitation (O'Hagan et al., 2006). By making expert judgments explicit and quantifiable, the subjective Bayesian framework allows for the synthesis of multiple independent lines of evidence, leading to more robust and scientifically coherent conclusions (Annan and Hargreaves, 2006). Therefore, the use of subjective priors is not an introduction of arbitrary bias, but rather a principled choice to ensure that statistical inference remains grounded in physical reality and leverages the full scope of scientific understanding.

The DOE CWG vastly simplifies this debate by repeating Lewis' critique of "evidence duplication" but failing to acknowledge the S20 authors' defense of their Bayesian methodology (Sherwood and Forest, 2024) or to note that S20 included extensive sensitivity analyses on their choice of priors. As such, the report uses a soundbite from complex scientific discussion as a rhetorical tool to cast doubt on a well-reasoned consensus rather than attempting to offer a balanced assessment. The goal of S20 and the IPCC was to make a general scientific assessment that requires synthesizing disparate lines of evidence—from paleoclimate proxies to satellite observations to complex model simulations—each with its own deep, unquantifiable uncertainties.

As such, in the case of S20, the expert prior is not an introduction of arbitrary bias, but a necessary tool to ensure the statistical result remains grounded in the full scope of physical understanding that cannot be captured by the simplified equations alone. The Jeffreys prior is derived solely from the structure of the chosen statistical model; if that model is an acknowledged simplification of reality, a prior based upon it is not necessarily more robust than one informed by the collective expert judgment of the field. Therefore, the subjective/expert guided Bayesian framework remains

a scientifically coherent and appropriate methodology for a task that is fundamentally about the synthesis of all available knowledge. In any event, it is acknowledged in Lewis (2022, 2024) that the adoption of objective priors has limited impact on the ECS median and confidence interval range (in fact both the range and median *increase* using an objective Jeffreys prior), such that these choices are broadly and quantitatively inconsequential to our understanding of ECS.

The role of the pattern effect

A crucial uncertainty in the quantification of ECS, in discrepancies between data-driven and model-driven ECS estimates, and between the aggregated estimates of Sherwood et al. (2020) and Lewis (2022), is the future evolution of sea surface temperatures in the equatorial Pacific Ocean. As the DOE CWG report notes [page 28 of the CWG], whether the historical spatial pattern of warming in the equatorial Pacific Ocean is consistent with the future spatial pattern of warming is critical in our overall estimate of ECS (a phenomenon known as the pattern effect). The DOE CWG report cites two studies that variously argue that models may misrepresent future warming patterns (Seager et al., 2019) and that the historical warming pattern may be expected to continue (Lee et al., 2024). They leverage these studies to argue that ECS is lower than implied by modelling studies, Sherwood et al. (2020), AR6, and the general scientific consensus. *[We note in addition that the citation in the DOE CWG for Lee et al. (2024) points to a paper that does not exist. The presumed correct citation, which has a different author list and journal information compared to the citation in the DoE CWG report, is "Lee, S., Bannon, P.R., Park, M., Clark, J.P., 2024. Zonal Contrasts of the Tropical Pacific Climate Predicted by a Global Constraint. Asia-Pac J Atmos Sci 60, 669–678. [https://doi.org/10.1007/s13143-024-00373-5]*

The report's handling of Seager et al. (2019) exemplifies this misrepresentation. The DOE CWG claims the paper shows that "contrary to models, the west-east temperature gradient has been strengthening over time" and that there is "no reason to expect the gradient to weaken" **[page 28]**. This is a direct contradiction to (Seager et al., 2019), who state "On decadal time scales, an overall stronger than normal SST gradient since 1998 has... **temporarily** reduced the rate of global warming in the atmosphere by enhancing the rate of ocean heat uptake" — thus not implying permanence of the enhanced temperature gradient (see the independent technical comment to the CWG on this matter by Clement et al. for additional context).

The DOE CWG report's choice to emphasize this single study, and over-emphasize their findings by omitting the authors own caveats, is an indicative case of cherry-picking from the scientific literature. In contrast, a large number of recent papers note the importance of the pattern effect in supporting the potential for a higher ECS than implied by historical observations (Alessi and Rugenstein, 2023; Andrews et al., 2018; Armour et al., 2013; Dong et al., 2021; Gregory and Andrews, 2016; Liang et al., 2024; Marvel et al., 2018; Marvel and Webb, 2025; Modak and Mauritsen, 2023; Myers et al., 2023; Olonscheck and Rugenstein, 2024; Rose et al., 2014). The DOE chapter dismisses this critical finding based on a narrow and contested interpretation of recent trends, failing to represent the full scope of the scientific debate and the physical reasoning behind the pattern effect adjustment in AR6. Several comprehensive reviews of all sides of this issue have been published, and were likewise not cited in the DOE CWG report (Lee et al., 2022; Watanabe et al., 2024).

The CWG also fails to include the full context from some of the cited sources for this debate in order to produce a narrative of false confidence in the assertion that pattern effects have had a

minimal influence on past temperature trends. For example, the CWG excludes the bold portion of the following quote from (Lee et al., 2024): "**While maxMEP solutions in general cannot provide a definite answer to the problem, these model results strengthen the possibility that** the trajectory of the observed trend reflects the response to increasing GHG loading in the atmosphere." Lee et al. (2024) also highlight the limitations of their idealized approach to make statements about real-world trends in their conclusions: "As was stated in the introduction, we view the maxMEP solution as a statistical inference rather than a definite answer. The prediction made by maxMEP is as good as how accurately our idealized climate system captures the essence of the real climate system. The model considered in this study does not, at least explicitly, include numerous processes that probably play a role in determining how the real tropical Pacific responds to GHG warming; ocean dynamics, moisture dynamics, planetary-scale wave dynamics, stratospheric dynamics, etc."

It is certainly the case that there is uncertainty about the future evolution of sea surface temperatures in the equatorial Pacific with major consequences for ECS and warming rates. However, the report's presentation of this issue represents a highly selective and misleading interpretation of the scientific literature. However, the authors of Forster et al. (2021) were fully aware of the warming pattern biases in current climate models. Their conclusion that the pattern effect is likely to increase warming was not made in ignorance of this fact but in spite of it. The AR6 assessment weighed multiple lines of evidence (Forster et al. 2021; Sherwood et al. 2020) :

- The physically-based understanding of how the tropical climate system responds to strong warming.
- The behavior of a wide ensemble of climate models.
- The historical record, including the understanding of its limitations in representing the forced response due to internal variability.

The consensus conclusion was that, for long-term projections, the physically-based mechanisms pointing to a future weakening of the gradient are a more robust guide than simply extrapolating a recent trend that is likely contaminated by natural decadal cycles. The DOE report presents this complex scientific judgment as a simple error, thereby misrepresenting the rigorous nature of the IPCC assessment process.

The report's narrow focus on the east-west tropical Pacific temperature gradient as the sole determinant of the pattern effect is also a critical oversimplification **[page 28]**. A physical mechanism for the increase of climate sensitivity over time has been proposed as the slow warming of ocean heat uptake regions, particularly the Southern Ocean and the North Atlantic (Armour et al., 2024). These high-latitude regions have weaker negative cloud feedbacks compared to the tropics, meaning that, as they eventually warm, the planet's overall climate sensitivity will rise as patterns of warming change (Rose et al., 2014). The DOE report's failure to engage with this primary mechanism indicates a superficial and biased assessment of the issue, while wider literature has examined this topic extensively (Watanabe et al., 2024).

Even within the flawed framework of focusing only on the tropical Pacific, the report's logic is unsound. For the pattern effect to cause a decrease in future ECS, the spatial pattern of warming would have to change. This would require the east-west temperature gradient to strengthen superlinearly relative to global warming (Dong et al., 2019). However, Lee et al. (2024), cited

extensively by the report [page 28], suggests the gradient scales linearly with forcing (e.g., see their Fig. 4). A linear scaling is consistent with a constant warming pattern, and therefore a constant ECS. The report thus cites evidence that contradicts its own implication that ECS might be lower than current estimates.

The DOE CWG extends the narrative of its assessment of IPCC ECS ranges to critique the CMIP6 ensemble, citing Scafetta (2022), which finds that most CMIP simulations warm too much over recent decades — and similar arguments have been made to dismiss the plausibility of high ECS models (Hausfather et al., 2022). However, this assessment ignores the role of the biased warming patterns in current generation models (Armour et al., 2024) which is likely to have suppressed observed warming, implying "observed warming is consistent with wide ranges of ECS and TCR extending to higher values than previously recognized." In short, the report fails to acknowledge that recent science has found differences between the patterns of warming seen in models and the real world, and these differences imply that past simulated global warming rates cannot be easily used to rule out models which warm a lot in the future.

Assumptions on aerosol forcing

The assumed strength of the cooling effect from aerosols is a critical 'lever' in any energy-budget estimate of climate sensitivity, and disagreements over this value explain a significant portion of the difference between recent studies. A weaker assumed aerosol cooling effect necessitates a lower climate sensitivity to explain the observed historical warming, and vice-versa (Smith et al., 2021). Sherwood and Forest (2024) point out that a key reason Lewis (2022) arrives at a lower Equilibrium Climate Sensitivity (ECS) is his "downward revision of expected historical aerosol cooling". In direct contrast, Wall et al. (2022) finds a "stronger mean of aerosol cooling and thereby a slightly higher ECS" from the same historical record. This demonstrates clearly that the resulting ECS estimate is directly dependent on the chosen aerosol forcing value.

The core of the issue is that the precise magnitude of the net cooling from aerosol-cloud interactions remains one of the largest uncertainties in climate science. Even recent studies are grappling with this complexity. For example, Sherwood and Forest (2024) note that the Wall et al. (2022) study, despite finding stronger cooling, is itself incomplete, as it "only considered sulfate effects on shallow clouds, which ignores potentially large effects from less well-understood cloud–aerosol interactions". This is consistent with the wider literature, including major reviews such as Bellouin et al. (2020) and more recent work like Quaas et al. (2022), which confirm that constraining the exact cooling effect from aerosols based on observations remains a frontier of research. Therefore, the choice of any single value or distribution for aerosol forcing is a significant judgment call by the authors that heavily influences the final climate sensitivity estimate. This is a real and important source of uncertainty that should not be eliminated by simply choosing a desired value.

While the DOE report verbally concedes that aerosol forcing is the "greatest source of uncertainty" in data-driven methods [page 27, CWG], its conclusions fail to represent this uncertainty adequately. The report correctly identifies the fundamental trade-off, stating that observed warming can be consistent with either "low ECS and low aerosol cooling, or high ECS and high aerosol cooling" [page 28, CWG]. However, having established this ambiguity, the chapter proceeds to build its entire narrative around evidence supporting only one side of that trade-off—the "low-low" scenario—by exclusively platforming analyses like Lewis (2022) that

rely on weaker aerosol cooling to derive a lower ECS. It does not give equal weight to the body of evidence supporting a stronger aerosol cooling effect, which would imply a higher ECS. In this way, the report uses the *existence* of uncertainty as a rhetorical tool to cast doubt on the consensus, while its own conclusions reflect a selective and unbalanced resolution of that same uncertainty.

Comment 3: Inadequate assessment of Policy-Relevant Metrics

Reference in CWG Report: General focus on ECS (pp. 25-28) and brief treatment of TCR (p. 29); omission of TCRE.

Comment Type: **Issue Selection Bias**. This addresses the report's strategic focus on an uncertain metric to the exclusion of more policy-relevant ones.

Original Text/summary statement from CWG Report: The chapter's near-exclusive focus is on the uncertainties and debates surrounding the Equilibrium Climate Sensitivity (ECS) metric. The report acknowledges that the Transient Climate Response (TCR) is a "more useful observational constraint".

Implementation: The critique will state that focusing almost entirely on the highly uncertain ECS metric is a form of bias that maximizes the appearance of scientific disagreement. It will highlight the **striking omission** of the **Transient Climate Response to Cumulative Emissions (TCRE)**, which is the scientific foundation for carbon budgets and the Paris Agreement.

Comment:

The DOE report dedicates the vast majority of its chapter to the uncertainties surrounding ECS. The arguments about model fitness, data-driven methods, the pattern effect and observational constraints are all focused on this single, long-term, and most uncertain metric. The report's bias is made clear by its own admission in its very brief section on the Transient Climate Response (TCR) which is acknowledged as a "more useful observational constraint" [page 29, CWG] than ECS and is "better constrained by historical warming" [page 29, CWG]. The decision to relegate the TCR metric to a short, concluding section, while focusing almost entirely on the more uncertain and less policy-relevant metric, is a clear example of issue selection bias. This framing allows the report to maximize the appearance of scientific disagreement and uncertainty, creating a misleading impression that the "consensus" view is in error or misleading.

Perhaps the most striking omission in the chapter — and the entire report — is the complete failure to mention the **Transient Climate Response to Cumulative Emissions (TCRE)** or the related **Zero Emissions Commitment (ZEC)**. The TCRE provides the direct, near-linear relationship between the total carbon ever emitted and the peak warming experienced (Allen et al., 2009; Matthews et al., 2009), and it is the scientific foundation upon which all carbon budgets are built (Damon Matthews et al., 2021; Jones and Friedlingstein, 2020). The ZEC concept confirms that global warming will largely cease once net-zero CO_2 emissions are reached, providing the physical basis for "net-zero" as a viable policy goal (Palazzo Corner et al., 2023). Together, these two concepts form the central scientific framework underpinning the entire Paris Agreement (Rogelj et al., 2019), as they are the internationally accepted tools used to translate temperature targets into

actionable emissions policies. For a report claiming to be a “Critical Review” of greenhouse gas impacts, to entirely ignore the primary scientific framework for international and national climate policy is an **inexplicable and scientifically unjustifiable omission.**

This biased framing stands in sharp contrast to the approach taken by Masson-Delmotte et al. (2023) and the USGCRP (2023), which provided a balanced treatment of all relevant metrics, using them to answer different questions. Crucially, in their policy-facing summaries (like the IPCC’s Summary for Policymakers), they give prominence to the TCRE relationship and the resulting carbon budgets. They do this because that is the most actionable and relevant information for policymakers working to meet specific warming targets like 1.5°C or 2.0°C. They do not share the DOE report’s selective and biased focus on ECS-related debates.

Comment 4: Climate model ECS is not generally ‘tuned’

Reference in CWG Report: Pages 26-27

Comment Type: Technical. This addresses the report’s mischaracterization of climate model development and its use in consensus assessments.

Original Text from CWG Report: "...the MPI modelers chose an ECS value of 3°C and then tuned the cloud parameterizations to match their intended result."

Implementation: Our critique will explain that the report uses the transparent practice of model "tuning" in one specific instance to imply a systemic flaw across all models. It will clarify that the IPCC process does not simply average model outputs but assesses the full ensemble in context with all other lines of evidence, explicitly accounting for known biases like the "hot model" problem.

Comment: The DOE report suggests the practice of "tuning" processes, which sets ECS in one specific model (Mauritsen and Roeckner, 2020), as evidence of a systemic flaw rather than transparency in the model development process, while also failing to acknowledge that model calibration is highly diverse amongst centers (Schmidt et al., 2017), and that the IPCC process (moreso than ever in AR6) assesses the full ensemble in context with other evidence, rather than assessing the distributions of outcomes from CMIP itself.

The report’s implicit assertion that climate models are "tuned" to specific ECS values is a faulty generalization and a mischaracterization based on a single example of the MPI model (Mauritsen and Roeckner, 2020) **[page 26]**. Firstly, in the MPI model, as for other Earth System Models, there is no single parameter for ECS. ECS is an emergent property, and is modulated by a set of interacting feedbacks in different components of the climate system. Some of the component representations in the model may have free parameters which could potentially change the strength of those feedbacks in the model, and therefore change the climate sensitivity (Sanderson et al 2008)

In the case of the MPI model, the ECS was not directly tuned to match a particular desired value, as the authors of (Mauritsen and Roeckner, 2020) note: “the target in the tuning was not a particular climate sensitivity, rather it was an improved match to the instrumental record, and

changing the climate sensitivity was a means to that end.”. As such, the model developers chose to adjust cloud parameters, which improved the historical warming simulation, and those parameters also influenced the diagnosed ECS of the model.

Secondly, if tuning to a desired ECS value was generally commonplace, the CMIP6 ensemble would not exhibit the well-documented "hot model" problem, where numerous models from diverse international institutions produced ECS values higher than previous IPCC assessed ranges. The very existence of this wide ECS spread in CMIP6 (Meehl et al., 2020) is therefore direct evidence against the concept that universal tuning of ECS is commonplace.

Furthermore, for climate risk assessment and constraint of real-world response, as well as to improve modeled historical warming representation, the ability to tune ECS to different levels would actually be useful, but this is in most cases a highly non-trivial exercise, given that relevant parameters are generally not *a priori* known without performing large parameter perturbation ensembles, and adjusting comparable parameters in different models has very different effects on ECS (Sanderson 2011). Furthermore, even if parameters which influence ECS can be identified, they often also exert strong influence on climatological performance (Peatier et al., 2022), which means that ECS cannot be easily altered without degrading the past and present performance of the model. In short, even if it were desired for specific purposes, tuning ECS is a highly complex undertaking, requiring dedicated non-standard experiments and huge computational resources to isolate the parameters that significantly influence ECS. Thus, contrary to what is implied in the DOE CWG report, this practice is not commonplace in model development.

In summary, the DOE report's suggestion that climate models are systematically rigged to a desired sensitivity is a false generalization. It mischaracterizes a single example where parameters were adjusted to improve historical performance, and those parameters also impacted ECS; i.e. ECS was not directly “tuned” as the report implies.. The wide diversity of results from different international modeling centers is direct evidence *against* a coordinated "tuning" effort. In reality, deliberately tuning for a specific climate sensitivity is technically very difficult, and is not a standard practice, nor is it a priority for model developers who are largely motivated with maximising the consistency of model simulations with observations (Schmidt et al., 2017, Mauritsen et al, 2012). The appropriate viewpoint for assessing ECS in the CMIP ensemble is to consider it as a broad, emergent feature of simulations that each attempt to faithfully reproduce the observed climate and its historical trends.

Comment 5: Omission of Current Observational Context

Reference in CWG Report: The entire chapter (pp. 25-30) lacks this context.

Comment Type: **Technical / Omission**. This addresses the report’s failure to connect its theoretical arguments to accelerating real-world observations.

Original Text/summary statement from CWG Report: The chapter’s entire discussion of climate sensitivity is framed through historical data and model disagreements, without reference to accelerating real-world warming trends.

Implementation: The critique will point out that the report's arguments for lower sensitivity are made in a vacuum, **ignoring the context of recent record-breaking global temperatures**, the planet's large and growing energy imbalance, and evidence that warming may be accelerating. This omission makes the report's narrative of complacency scientifically unjustifiable.

Comment:

The DOE report's intense focus on a precise value for the long-term, multi-century metric of ECS is a distraction from the more immediate evidence that the climate system is presently warming rapidly. Global temperatures are approaching 1.5°C and already likely have exceeded this level during a 12 month period (Bevacqua et al., 2025; Cannon, 2025), with some evidence that the rate of warming has accelerated (Samset et al., 2023). This recent acceleration follows a period where a specific pattern of sea surface temperatures after 1998 may have temporarily suppressed surface warming by increasing the reflection of sunlight from low clouds (Zhou et al., 2016). However, evidence suggests this period is over, with the pronounced warming of 2023–2024 being associated with a large reduction in the reflection of shortwave radiation from clouds (Goessling et al., 2025). This is happening while the planet maintains a large and growing positive energy imbalance, with the Earth system continuing to accumulate heat which commits the planet to further warming (Hodnebrog et al., 2024; von Schuckmann et al., 2023). Therefore, the DOE report's focus on historical disagreements to support a lower-end sensitivity estimate fails to include the context of recent observational global temperature trends.

Growing evidence for the intensification of global warming over recent years increasingly challenges conclusions about the magnitude of climate sensitivity that were derived from the comparatively slower warming rate over prior decades. While the formal incorporation of these recent trends into long-term ECS estimates is an ongoing process, absence of any discussion of recent accelerations in warming in any "Critical Review" is a significant flaw.

Recommendations

To address the aforementioned errors, omissions, and bias, the authors of the DOE CWG report need to:

1. ***Remove assertions of a dichotomy between model and data-derived ECS.*** - They should instead acknowledge and highlight the widely adopted and scientifically robust approach of recognising all available information in concert, while assessing uncertainties in each. The revision must acknowledge that the scientific consensus on Equilibrium Climate Sensitivity (ECS), as established by the IPCC AR6 and other major assessments, is not based on choosing one method over the other. Instead, its strength lies in the synthesis of multiple, independent lines of evidence, including climate models, historical observations, paleoclimate records, and process understanding. The report should be amended to reflect that models are used as crucial tools to test physical theories, and are not used as calculators of ECS.

2. Correct selective citation of literature that supports their preferred conclusion of a low ECS. - The current text selectively cites literature that supports a low-ECS narrative while downplaying or omitting contradictory evidence. To correct this, the report must incorporate the full scientific debate on key uncertainties, particularly:

- *Use of priors:* The report should avoid misrepresenting the use of subjective Bayesian priors in consensus assessments as a "methodological error." Instead, it should acknowledge the ongoing and nuanced debate in statistical philosophy and consider the explicit sensitivity analyses performed in such assessments, rather than selectively critiquing their application to support a specific narrative.

- *Aerosol Forcing:* Acknowledge that the magnitude of aerosol cooling is a primary source of uncertainty and that assuming a weaker cooling effect (which leads to a lower ECS) is a critical judgment call, not a settled conclusion.

- *Pattern Effect:* The dismissal of the "pattern effect" based on a narrow interpretation of recent trends is highly misleading as it excludes a major source of uncertainty about how historical warming rates can be accurately used to quantify climate sensitivity. The revision should include the significant body of evidence and physical reasoning suggesting that historical warming patterns have likely suppressed observed warming, meaning ECS could be higher than historical data alone suggest.

3. Broaden the focus to include policy-relevant metrics - The report's near-exclusive focus on the uncertain, long-term ECS metric is a form of issue selection bias that exaggerates scientific controversy by highlighting a metric which has only minimal policy relevance. The document must be revised to include a thorough discussion of more policy-relevant metrics that are better constrained by observations. This includes not only the Transient Climate Response (TCR), which the report itself calls a "more useful observational constraint", but also, most critically, the Transient Climate Response to Cumulative Emissions (TCRE) - which is the quantity which underpins understanding of how remaining carbon budgets relate to warming thresholds.

4. Explicitly acknowledge and address how model biases are addressed in assessments. The report should refer to the fact that, in IPCC AR6, inconsistencies between models and observations and potential biases in model climate sensitivity values are taken into account in assessments of future climate change. The CMIP6 historical simulations assessed in IPCC AR6 have an ensemble mean global surface temperature change within 0.2°C of the observations over most of the historical period, and observed warming is consistently captured by the CMIP6 model spread (Eyring et al., 2021). Biases are assessed to be small enough to support detection and attribution of human-induced warming (very high confidence). IPCC AR6 clearly notes that some CMIP6 models simulate a warming that is either above or below the assessed very likely range of observed warming. However, this information about how well models simulate past warming, as well as other insights from observations and theory, are used to assess projections of global warming (see Cross-Section Box TS.1 in (Arias et al., 2023)).

5. **Show consistency in discussing the implications of sensitivity.** The report makes clear that sensitivity metrics are useful for assessing the danger of CO_2 pollution and the need for "climate action," but are internally inconsistent about the implications of the metrics they assess, claiming that 2ºC ECS is the threshold below which emissions controls are unnecessary (Dayaratna et al., 2017, 2020), but then that ECS is likely above 2ºC (Lewis, 2022). In addition to using more policy-relevant metrics for the danger of CO_2 (such as TCRE; see point 3 above), the authors should be explicit in their assessments of these metrics and their relation to the thresholds for societal danger.

Suggested update to summary statements

Given the above we suggest the following update to the Chapter 4 summary statements:

- **On the Scientific Assessment of Climate Sensitivity**: The scientific consensus on Equilibrium Climate Sensitivity (ECS), as established by the IPCC AR6, is based on **synthesizing multiple independent lines of evidence**, including climate models, historical observations, and paleoclimate records. This approach avoids relying on any single method, using climate models as essential tools for understanding physical processes and feedbacks rather than as direct calculators of ECS. While each line of evidence has uncertainties, their combination provides a more robust assessment than any individual method alone.
- **On the Assessed Range of Climate Sensitivity**: The IPCC AR6 assessed a likely ECS range of 2.5°C to 4.0°C, narrowing the uncertainty by constraining both the high and low ends. This was achieved by combining evidence from paleoclimate, which makes very high sensitivity unlikely, with historical warming and process understanding, which make very low sensitivity unlikely. This assessment accounts for major uncertainties, such as the magnitude of aerosol cooling and the "pattern effect," acknowledging that selectively downplaying these factors can lead to biased, artificially low ECS estimates.
- **On Policy-Relevant Climate Metrics**: While ECS is a useful metric, climate policy and near-term warming projections are better informed by metrics like the Transient Climate Response (TCR) and the **Transient Climate Response to Cumulative Emissions (TCRE)**. The TCRE, which establishes a direct link between total CO_2 emissions and global warming, is the fundamental scientific basis for carbon budgets and international climate targets like the Paris Agreement. A complete assessment of climate sensitivity must include these policy-relevant metrics.

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Technical Comment on Page 28 , Section 4.3 'Data-driven estimates of climate sensitivity'

By Amy Clement, Ph.D., Richard Seager, Ph.D., Adam Sobel, Ph.D., Mark Cane, Ph.D., Jeremy Klavans Ph.D.

The DOE report cites Seager et al. (2019) in Section 4.3 to counter AR6's claim that a weakening tropical Pacific SST gradient would raise climate sensitivity. They say that the gradient has actually strengthened, likely due to misrepresented ocean dynamics in models, implying climate sensitivity could be lower than current estimates.

However, the Seager et al. (2019) finding, that the Pacific equatorial sea surface temperature gradient has increased in the real world but not models, does not support a lower estimation of equilibrium climate sensitivity (ECS) than those produced by the models. It is not clear that this increased gradient will persist in the future especially as the temperature of subsurface waters that upwell at the Equator increases (Held 2010, An et al. 2012, Heede et al. 2021). Even if the strengthened gradient does persist it is not clear what its effect is on clouds and water vapor and, therefore, ECS. There are competing effects of local versus tropics-wide processes on equatorial cloud cover (e.g. Klein and Hartmann 1993, Myers and Norris 2013) and humidity (e.g. Pierrehumbert 1993, Clement and Seager 1999), and it is unknown how these will respond to the particular pattern of Pacific equatorial cooling amidst tropical warming (Jiang et al. 2024). Thus, while the strengthened gradient could imply a reduced *transient* climate response, how long it lasts, and how it impacts the *equilibrium* climate sensitivity is not known.

What is more consequential is that persistence of the strong gradient noted by Seager et al (2019) is likely to cause worsening drought in the Southwest United States (Kuo et al. 2025, Seager et al. 2023) and increasing Atlantic hurricane activity (Sobel et al. 2023), based on historical associations between these impacts and La Niña events as well as longer-term trends. These well-established and consequential linkages from the tropical Pacific are not cited in the DOE report.

In summary, the observed trend in the equatorial temperate gradient is not guaranteed to persist long into the future while its impacts on clouds and water vapor are poorly known. Therefore its effect on the future warming rate is unknown. The uncertainty in the persistence of this trend means that it cannot be used to imply a lower estimate of ECS (which relies on future temperature trends). However, it is clear that the strengthened equatorial gradient in response to anthropogenic forcing drives significant and costly alterations to U.S. climate via its impact on drought and hurricanes. These points should be included in the report, including the implications for the main findings of the report.

We also note that the authorship list in the cited Seager et al. (2019) is not correct. The correct citation is below.

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Section 5.2: Surface Warming

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Summary: The DOE CWG report rightly acknowledges that it is important to benchmark model simulations of surface warming against historical observations. The authors argue that climate models, except for climate models with low climate sensitivity, overestimate surface warming from 1979 to the near-present. This incorrect claim is referenced in the DOE CWG Report's Executive Summary to conclude that models produce "exaggerated projections of future warming" (DOE CWG pg. ix). This conclusion relies on incorrect claims, ignores scientific research that helps to explain differences in model-versus-observed warming, and relies on a misleading framing that model projections are the sole source of information about future changes in climate.

The following changes must be made to Section 5.2 in the DOE CWG document and to related statements in the executive summary and chapter summary:

- **The incorrect claim that only models with the lowest ECS reproduce observed warming must be removed.**
- **Revise Figures 5.2 and 5.3 and related statements in the report to (a) use a longer reference period to compute the anomalies to ensure the influence of internal variability is minimized in model-observational comparisons (e.g. reference period 1850–1900 as in IPCC AR6), (b) show the full historical record so that model performance can be evaluated beyond the satellite era, (c) account for observational uncertainty, and (d) include representation of internal variability from the models by showing the range of anomalies and trends from individual model ensembles**.
- **The potential role of forcing biases on simulated-versus-observed warming differences must be discussed.**
- **Include statements on warming biases of climate models over longer time periods.** The CMIP6 historical simulations assessed in IPCC AR6 have an ensemble mean global surface temperature change within 0.2°C of the observations over most of the historical period, and observed warming is within the 5–95% range of the CMIP6 ensemble. Model biases are assessed to be small enough to support detection and attribution of human-induced warming (very high confidence).
- **Remove the statement on "exaggerated projections of future warming" because this assertion is an oversimplification: IPCC assessment projections rely on more**

> **than raw climate model output.** The report should refer to the fact that in IPCC AR6, inconsistencies between models and observations and potential biases in model climate sensitivity values are taken into account in assessments of future climate change. IPCC AR6 clearly notes that some CMIP6 models simulate warming that is either above or below the assessed very likely range of observed warming. However, this information about how well models simulate past warming, as well as other insights from observations and theory, are used to assess projections of global warming (see Cross-Section Box TS.1 in Arias et al. (2021)).

These issues and necessary corrections are further detailed below.

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The authors focus on a subset of the historical period, which results in a misleading comparison of models and observations. A comparison of simulated-versus-observed warming is discussed in Eyring et al. (2021) and provides greater context:

> Papalexiou et al. (2020), Tokarska et al. (2020) and Stolpe et al. (2021) all report that CMIP6 models on average overestimate warming from the 1970s or 1980s to the 2010s, although quantitative conclusions depend on which observational dataset is compared against [...]. However, Figure 3.4 [reproduced below as Figure 1], which includes a larger number of models than available to those studies, indicates that the CMIP6 multi-model mean tracks observed warming better than the CMIP5 multi-model mean after the year 2000. The CMIP6 multi-model mean GSAT warming between 1850–1900 and 2010–2019 and associated 5–95% range is 1.09 [0.66 to 1.64] °C. [...] So some CMIP6 models simulate a warming that is smaller than the assessed observed range, and other CMIP6 models simulate a warming that is larger. That overestimated warming may be an early symptom of overestimated ECS in some CMIP6 models ([...] Meehl et al., 2020; Schlund et al., 2020), and has implications for projections of GSAT changes ([...] Liang et al., 2020; Nijsse et al., 2020; Tokarska et al., 2020; Ribes et al., 2021).

While Eyring et al. (2021) acknowledge that some models may overestimate warming because they have climate sensitivity values that are too large, climate models from the Coupled Model Intercomparison Phase 6 (CMIP6) track with observations over the long-term (see Figure 1 from IPCC AR6 Fig. 3.4). This context is important because warming trends over shorter decadal or multidecadal time periods may include a substantial contribution from internal climate variability (Figure 2), which can temporarily enhance or diminish observed trends relative to the average warming simulated in models.

To correct this issue, the DOE CWG Report must include simulated and observed time series of global surface temperature anomalies over the full historical record (1850 to present) so that model concordance with observations can be assessed outside the satellite era. To mitigate the potential influence of internal variability when comparing simulated and observed time series, a sufficiently long reference period (e.g., 1979 - 2008 and 1850 - 1900) should be used. The DOE CWG report should include a balanced assessment about model performance over the full historical record, clearly noting that the CMIP6 historical simulations assessed in IPCC AR6 have an ensemble mean global

surface temperature change within 0.2°C of the observations over most of the historical period, and observed warming is within the 5–95% range of the CMIP6 ensemble.

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The comparison between models and observations in the DOE CWG Report does not account for factors that help to explain differences between simulated and observed warming nor does it discuss potential flaws in ascribing model-observational differences solely to climate sensitivity biases. Research indicates that internal variability has reduced global warming over 1979 to the near-present (Po-Chedley et al. 2021, 2022; Sweeney et al., 2023). These effects are important when assessing projections of future warming. Statistical constraints on climate warming projections support greater future warming when internal variability in observations is accounted for (Liang et al. 2024). There is also evidence that discontinuities in model prescribed biomass burning aerosols has resulted in exaggerated warming in some models over recent decades (Fasullo et al. 2022; Po-Chedley et al. 2022). Another shortcoming with Section 5.2 of the DOE CWG Report is that the authors ignore uncertainty in observed trends, which makes it impossible to fairly assess model-observational trend compatibility (Schmidt et al. 2023). Despite the effects of internal variability, which appear to have reduced observed warming, and forcing biases, which may have enhanced model simulated warming, models with low, medium, and high climate sensitivity values are able to reproduce observed satellite era surface warming within observational uncertainty (see Figure 2, Fig. 3 in Tokarska et al. 2020, and Schmidt et al. 2023), which is contrary to the DOE CWG statement (page 31) that only models with the lowest ECS reproduce observed trends. In fact, in DOE CWG Figure 2 models with trends below the mean of the observations have climate sensitivity values as high as 4.9 K (Zelinka et al. 2020). The model-observational comparisons in Section 5.2 of the DOE CWG document are problematic because they do not account for internal variability, observational uncertainty (see, e.g., Menemenlis et al. 2025), and forcing biases.

To correct these issues, the DOE CWG Report must include estimates of observational uncertainty and include representation of internal variability from the models by showing the range of trends from model ensembles (e.g., in DOE CWG Figure 5.3). The authors must discuss the potential effects of forcing biases on simulated warming trends. The incorrect claim that only models with the lowest ECS reproduce observed warming must be removed.

Page 31, 33, and page ix: Technical Comment

In terms of framing, the DOE CWG report portrays models as “overly sensitive,” which contributes to “exaggerated projections of future warming” (DOE CWG page ix). As already noted above, this characteristic applies to a subset of climate models and their projections have been downweighted in the IPCC AR6 assessment and other analyses (Hausfather et al., 2022). The DOE CWG report's statements on climate sensitivity must also be placed in the context of more comprehensive assessments of climate sensitivity that use many lines of evidence (e.g., Sherwood et al. 2020; Forster et al. 2021, Myers et al. 2021). In fact, recent research indicates

that analysis of satellite-era warming trends produces constraints on climate sensitivity and future climate projections that are biased low and overconfident (Armour et al. 2024). Moreover, the DOE CWG report's claim implies that raw climate model projections are the only source of data used to inform our expectations of future climate change. In reality, the IPCC report produces projections that are based on multiple lines of evidence, including climate models, assessed values of climate sensitivity, and observational constraints (Lee et al. 2021; Hausfather et al. 2022). In other words, inconsistencies between models and observations, as well as potential biases in model climate sensitivity, are explicitly taken into account in assessments of future climate change. These important considerations are entirely overlooked in the DOE report and should be corrected.

To correct these issues, the DOE CWG Report must note that individual analyses offer a poor constraint on climate sensitivity and future projections. The report must refer to the fact that in IPCC AR6, inconsistencies between models and observations and potential biases in model climate sensitivity values are taken into account in assessments of future climate change. Information about how well models simulate past warming, as well as other insights from observations and theory, are used to assess projections of global warming (see Cross-Section Box TS.1 in Arias et al. (2021)).

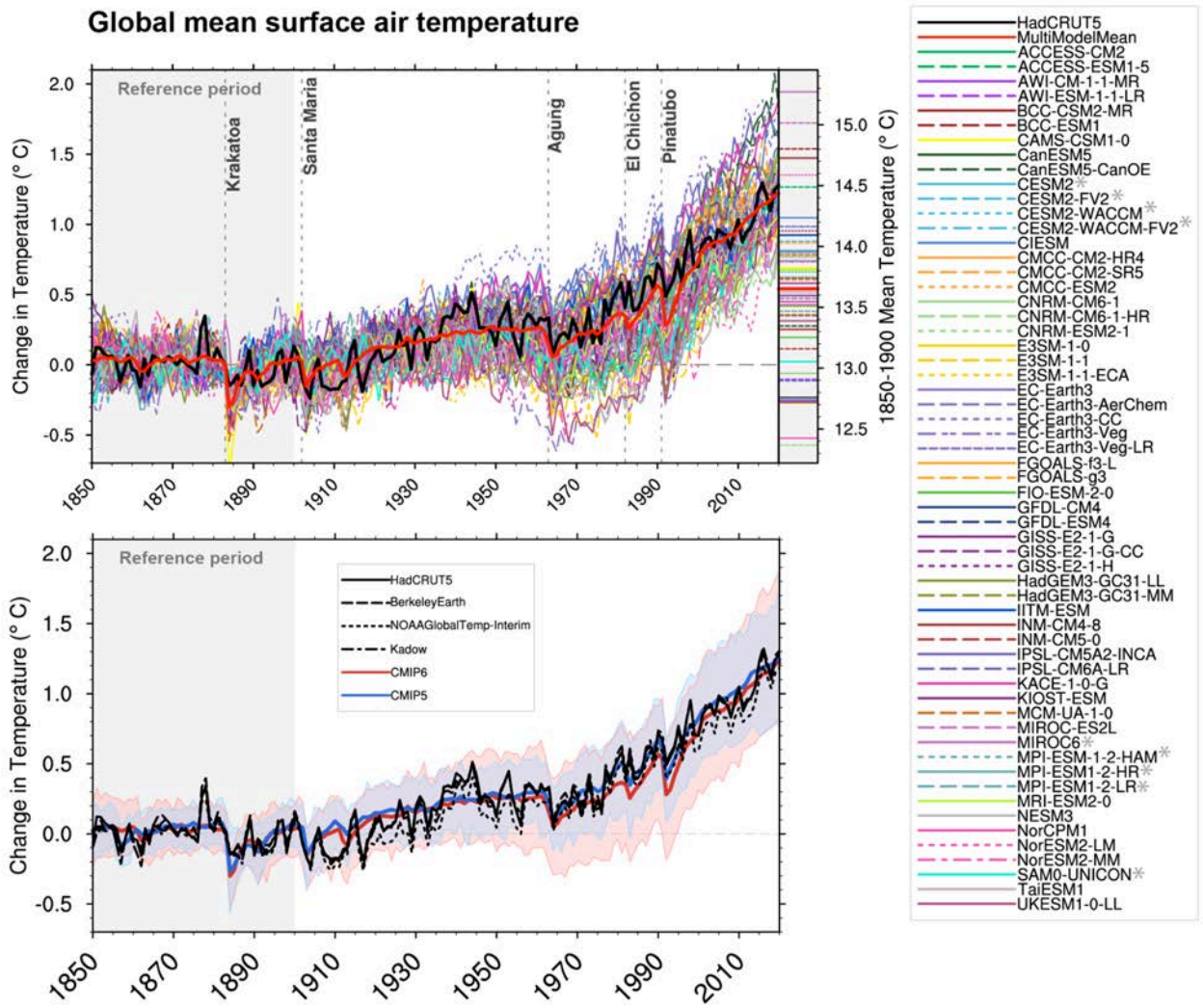

Figure 1 reproduced from IPCC AR6 Figure 3.4: Observed and simulated time series of the anomalies in annual and global mean surface air temperature (GSAT). All anomalies are differences from the 1850–1900 time-mean of each individual time series. The reference period 1850–1900 is indicated by grey shading. (a) Single simulations from CMIP6 models (thin lines) and the multi-model mean (thick red line). Observational data (thick black lines) are from the Met Office Hadley Centre/Climatic Research Unit dataset (HadCRUT5), and are blended surface temperature (2 m air temperature over land and sea surface temperature over the ocean). All models have been subsampled using the HadCRUT5 observational data mask. Vertical lines indicate large historical volcanic eruptions. CMIP6 models which are marked with an asterisk are either tuned to reproduce observed warming directly, or indirectly by tuning equilibrium climate sensitivity. Inset: GSAT for each model over the reference period, not masked to any observations. (b) Multi-model means of CMIP5 (blue line) and CMIP6 (red line) ensembles and associated 5th to 95th percentile ranges (shaded regions). Observational data are HadCRUT5, Berkeley Earth, National Oceanic and Atmospheric Administration NOAAGlobalTemp-Interim and Kadow et al. (2020). Masking was done as in (a). CMIP6 historical simulations were extended with SSP2-4.5 simulations for the period 2015–2020 and CMIP5 simulations were extended with RCP4.5 simulations for the period 2006–2020. All available ensemble members were used (see Section 3.2). The multi-model means and percentiles were calculated solely from simulations available for the whole time span (1850–2020). Figure is updated from Bock et al. (2020), their Figures 1 and 2. CC BY 4.0 https://creativecommons.org/licenses/by/4.0/. Further details on data sources and processing are available in the chapter data table (Table 3.SM.1).

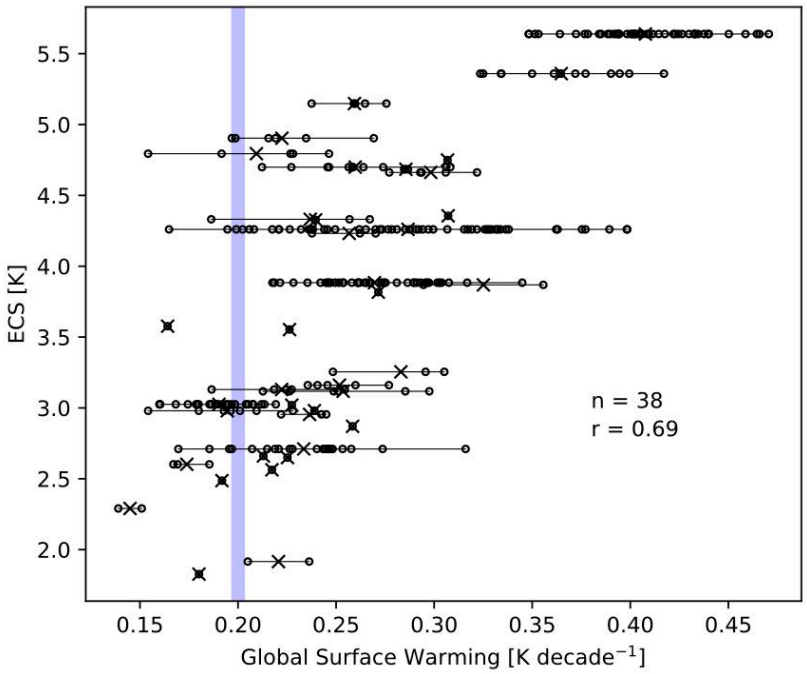

Figure 2: Global surface air temperature trends (1979-2024) in CMIP6 models (historical simulation over 1979-2014 and ssp370 over 2015-2024) versus model equilibrium climate sensitivity (ECS) values. Each open circle is an individual model member. When a model has multiple members, the ensemble mean is marked with an X and the range of trends is denoted with a horizontal black line. The range of observed surface trends from observations is denoted with the vertical blue line (across HadCRUT5, GISTEMPv4, BEST, and NOAAGlobalTempv6). The number of models (n) and correlation (r) across model ECS and model ensemble mean surface warming is noted in the figure. Note that the effects of a) "blending" sea surface and surface air temperatures and b) structural uncertainties from varied SST products were not accounted for in this comparison (Tokarska et al. 2020; Menemenlis et al. 2025).

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Section 5.3: Tropospheric Warming

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Summary: Comparisons between simulated and observed tropospheric temperature trends have received considerable scientific attention for decades. The heat transfer between the surface and the tropical troposphere is a critical component of the climate system and an important metric for evaluating climate models. While most model simulations exhibit more tropical and global tropospheric warming than observations, there are a number of factors that affect this model-observational comparison and it is premature to conclude that models are substantially deficient given the large uncertainties in observational estimates of atmospheric temperature trends, the role of internal variability, and the potential role of model forcing biases. The DOE CWG's discussion of this issue requires revision.

The following changes must be made to Section 5.3 in the DOE CWG document and to related statements in the executive summary and chapter summary:

- **Reference and discuss research that examines the role of humans in affecting tropospheric temperature trends.**
- **Include discussion of the large uncertainties associated with tropospheric temperature trend estimates and the potential role of observational biases in explaining model-observational differences in tropospheric warming rates. Internal dataset uncertainties and structural uncertainties should be included in graphics comparing trends from models and observations (DOE CWG Figure 5.4).**
- **Include representation of internal variability from the models by showing the range of trends from model ensembles (DOE CWG Figure 5.4). The potential effects of forcing biases on simulated warming trends must be discussed.**
- **Accurately reference to the IPCC AR6 statement that there is medium confidence that models "overestimate observed warming in the upper tropical troposphere over the 1979–2014 period *by at least 0.1°C per decade*", contextualized with discussion of the impacts of internal variability, forcing biases, and observational uncertainties on model-observational trend differences.**

Page 34, Technical Comment

The authors assert that:

> It has long been known that climate models on average overstate warming in the tropical troposphere. This region is an important test of climate models since this is where the signal of anthropogenic greenhouse warming emerges first and most strongly.

The suggestion in the DOE CWG Report that the tropical tropospheric temperature trend is a primary signal of anthropogenic climate change is an incorrect, strawman argument. In the tropics, the surface-troposphere system is strongly coupled by convection, which acts to maintain a "stable" vertical temperature structure. This strong coupling and moist thermodynamic processes (Stone and Carlson, 1979) mean that surface warming is accompanied by tropospheric warming regardless of whether surface warming is a result of anthropogenic or natural causes. Furthermore, the tropical troposphere is not "where the signal of anthropogenic greenhouse warming emerges first and most strongly." The signal of human-caused CO_2 increases actually emerges first and most strongly in the mid- to upper stratosphere (Santer et al., 2023, 2025).

Attribution of climate trends generally leverages the spatial patterns of changes, and requires comparison of long-term trend patterns (signals) relative to the patterns of internal climate variability. Comprehensive detection and attribution analyses have demonstrated that the strength and pattern of simultaneous and sustained global-scale tropospheric warming and stratospheric cooling result from anthropogenic influence (Santer et al., 2017, 2023; Romanzini-Bezerra and Maycock 2024). Natural causes cannot produce this pattern.

To address this comment, the authors must correct the false statement that one metric, tropical tropospheric warming, is "is where the signal of anthropogenic greenhouse warming emerges first and most strongly." The authors must refer to relevant studies regarding the human impact on the vertical structure of atmospheric temperature, from the lower troposphere to the upper stratosphere (Santer et al., 2023, 2025). Such studies find that anthropogenic influence has significantly affected atmospheric temperature trends, far beyond levels that could plausibly be explained by natural causes.

Page 35-36, Technical Comment

The DOE CWG Report highlights that models "overstate warming in the tropical troposphere" and that:

> The discrepancy was flagged as a serious inconsistency in the first U.S. Climate Change Science Program report (Karl et al. 2006) and has been mentioned in every IPCC report since, but the discrepancy has gotten worse over time, and the bias is now global.

While the DOE CWG Report focuses on model biases as the cause of model-observational differences, the US CCSP Report (Karl et al. 2006) actually points out that such model-observational discrepancies may arise from "errors that are common to all models, from

errors in the observational data sets, or from a combination of these factors. The second explanation is favored, but the issue is still open." The CCSP Report's emphasis on a role for observational biases was ignored in the DOE CWG Report. Evidence from the timescale-invariance of vertical amplification behavior and from complementary variables (like atmospheric moisture) suggests that observations may underestimate tropical tropospheric warming (Santer et al. 2005, 2021; Mears et al., 2007).

Observational biases and uncertainty continue to be a substantial concern for estimates of tropical and global tropospheric warming derived from satellite-based microwave sounders, which require the removal of significant non-climatic artifacts. Although uncertainty estimates for observations of tropospheric warming tend to be small (e.g., 0.05 K decade^{-1} or less; Christy et al. 2003 and Mears et al. 2011), dataset updates frequently result in non-negligible trend updates that can sometimes be as large or larger than dataset uncertainty estimates (e.g., Mears and Wentz 2005; Christy and Spencer 2005; Mears et al. 2016; Zou et al. 2023). For example, a 2023 update applied to the National Oceanic and Atmospheric Administration Center for Satellite Applications and Research dataset (v4.1 to v5.0) resulted in a decrease in global (tropical) tropospheric temperature trends from 0.16 to 0.09 (0.23 to 0.14) K decade^{-1} (Zou et al. 2023; Po-Chedley et al. 2023). Similarly, trend estimates from different research groups often differ significantly, even though the datasets are derived from the same raw data.

Radiosonde (weather balloon) and reanalysis-based observations likewise are subject to difficult to estimate and difficult to remove biases in their estimate of tropospheric warming (Titchner et al. 2009; Dee and Uppala 2009; Thorne et al. 2011; Haimberger et al. 2012; Sherwood et al. 2015). Homogenizing long-term radiosonde temperature datasets is challenging because of the relatively sparse, time-varying spatial coverage of records, systematic differences in the instrument types in different countries, changes in instrumentation as a function of time, and incomplete metadata. These challenges make it difficult to fully remove biases that affect temperature trends or to accurately quantify such biases (Titchner et al. 2009).

To address these issues, the DOE CWG Report must note that research, including assessment reports such as the CCSP Report, highlight a role for observational biases in explaining model-observational differences in tropospheric warming rates. The authors must also include discussion of the large uncertainties associated with tropospheric temperature trend estimates. Internal dataset uncertainties and structural uncertainties should be included in graphics comparing trends from models and observations (DOE CWG Figure 5.4). The authors should also discuss the potential for unaccounted for observational biases.

Page 35, Technical Comment

The DOE CWG Report asserts that "Biases in tropospheric trends indicate model flaws in heat transfer processes that carry over to surface warming biases." This statement ignores other possible contributors to differences in simulated and observed tropospheric warming. Aside from observational biases and uncertainty (discussed above), the DOE CWG Report also fails to

account for contributions of internal variability and forcing biases in explaining some of the differences in model-versus-observed warming rates (Mitchell et al. 2020; Fyfe et al. 2021; Po-Chedley et al. 2022; Casas et al. 2022). For example, several studies have pointed out that internal variability has likely reduced global and tropical tropospheric and surface warming over the satellite-era (Po-Chedley et al. 2022; Sweeney et al., 2023). Such multidecadal leaves an imprint on the pattern of surface warming; when climate models by chance exhibit a similar manifestation of internal variability to the real word, they produce a similar pattern of surface warming and trends that are in better accord with observations (Po-Chedley et al. 2021). The DOE CWG Report does not discuss the role of internal variability issues nor does it account for variability in its update to McKitrick and Christy (2020) (DOE CWG Figure 5.4). In this analysis, the authors only sample one simulation per model, even though the effect of internal variability can be substantial over this time period, enabling models with high climate sensitivity to produce simulations with trends near to or in the range of observations (Po-Chedley et al. 2021; Casas et al. 2022). Research also suggests that the forcings prescribed to models have substantial uncertainties (Fyfe et al. 2021; Holland et al. 2024). For example, discontinuities in model prescribed biomass burning aerosols has resulted in exaggerated warming in some models over recent decades (Fasullo et al. 2022; Po-Chedley et al. 2022). Studies have also shown that missing or incorrectly specified forcings can affect the consistency between simulated and observed tropospheric temperature trends (e.g., Santer et al., 2014, 2017b). Internal variability and forcing biases, in addition to climate sensitivity, must be accounted for when comparing simulated and observed tropospheric warming.

To correct these issues, the DOE CWG Report must include representation of internal variability from the models by showing the range of trends from model ensembles (DOE CWG Figure 5.4). The authors must discuss the potential effects of forcing biases on simulated warming trends.

Page 36, Technical Comment

The DOE CWG Report concludes this section noting that, “despite the accumulation of evidence of excess model warming the IPCC assigns only medium confidence to the existence of a warming bias.” The suggestion is that the IPCC has understated the warming bias, but the actual statement in the IPCC Report is more specific: “we assess with medium confidence that CMIP5 and CMIP6 models continue to overestimate observed warming in the upper tropical troposphere over the 1979–2014 period by at least 0.1°C per decade, in part because of an overestimate of the tropical SST trend pattern over this period.” There is evidence that when internal variability, forcing, and observational biases are accounted for, CMIP models do not significantly exceed observed tropical or global tropospheric warming (particularly by such a large magnitude), especially for models with ECS values within the IPCC likely range of 2.5 to 4 K (Santer et al. 2021; Po-Chedley et al. 2022).

To address these issues, the DOE CWG Report accurately refer to the IPCC AR6 statement that there is medium confidence that models “overestimate observed warming in the upper tropical troposphere over the 1979–2014 period by at least 0.1°C per

decade". As noted above the authors must also discuss the impacts of internal variability, forcing biases, and observational uncertainties on the significance of model-observational trend differences.

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Section 5.4: Vertical Temperature Profile Mismatch

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Summary: Section 5.4 of the DOE CWG Report notes that the simulated and observed atmospheric temperature trends differ, which is likely true over some regions, altitude levels, and time periods. This issue was evaluated in the IPCC AR6 Report: “[W]e assess with medium confidence that CMIP5 and CMIP6 models continue to overestimate observed warming in the upper tropical troposphere over the 1979–2014 period by at least 0.1°C per decade, in part because of an overestimate of the tropical SST trend pattern over this period.” Although there is evidence that model biases play a role in shaping model-observation differences in the vertical profile of atmospheric temperature change, the influence of internal variability and the considerable structural uncertainty within the observational record preclude any firm conclusion with high confidence. Furthermore, it is not clear how a model bias in the profile of tropospheric temperature change would affect other aspects of the Earth system such as the climate sensitivity. The relative contributions of observational uncertainty, internal variability, errors in the prescribed model forcing, and model biases to the model-observation discrepancies are likewise unresolved. By omitting discussion of these uncertainties and overlooking evidence that does not align with its conclusions, the DOE CWG Report presents an incomplete and unbalanced account of the issue.

Particularly, the following changes must be made to Section 5.4 in the DOE CWG document:

- **Present peer-reviewed research that demonstrates that long-term tropospheric temperature trends are inconsistent with internal climate variability.**
- **The conclusion of Section 5.4 that the IPCC AR6 did not assess the atmosphere’s temperature profile and fundamental feedback processes is misleading and needs to be revised.** See for example IPCC AR6 Figure 3.10 on observed and simulated tropical mean temperature trends through the atmosphere.
- **Revise Figure 5.6 and related statements in the report to consider internal variability and to account for observational uncertainty**. **The potential role of model prescribed forcing biases should also be discussed.**
- **The simulated upper tropospheric temperature trends should be compared to GPS RO data, given the stability of GPS RO derived temperature observations.**

Page 36, Technical Comment

The authors note that observed trends from balloon-borne thermometers (radiosondes) appear to be consistent with natural forcings, though they omit that the figure cited (DOE CWG Figure 5.5) was adapted from Lott et al. (2013), which concluded that: “With increased confidence over previous studies, human influences (both greenhouse gas and other anthropogenic forcings) have been detected in spatiotemporal changes in free atmosphere temperature from 1961 to 2010, irrespective of whether the full atmospheric column (30–850 hPa) is examined or purely the troposphere, with stratospheric ozone depletion dominating the cooling that has been observed in the lower stratosphere.” The interpretation in the DOE CWG Report that observed atmospheric temperature trends are consistent with internal variability is also at odds with other comprehensive studies that demonstrate that the magnitude and structure of atmospheric warming is inconsistent with internal and natural climate variability (Santer et al. 2013; 2017b, 2023).

To correct this issue, the authors should reference and discuss peer-reviewed research that finds that long-term tropospheric temperature trends are inconsistent with internal and natural climate variability (e.g., Lott et al. 2013; Santer et al. 2013; 2017b).

Page 37, Technical Comment

The DOE CWG Report includes an update of the model-versus-observation vertical temperature trend profiles from Christy and McNider (2017). The updated figure uses reanalysis data as a stand-in for observations (the original analysis used radiosondes). From their figure (DOE CWG Figure 5.6) we see that there are substantial structural uncertainties in the data, especially in the upper troposphere. For instance, the JRA-3Q reanalysis overlaps with some model estimates of upper tropospheric warming while ERA5 tends to overlap with the models at some lower tropospheric levels. Aside from these structural uncertainties, it is important to recognize that reanalyses contain drifts and discontinuities that affect atmospheric temperature trends (Dee and Uppala. 2009; Hersbach et al. 2020).

Determining the vertical structure of tropical tropospheric temperature trends from radiosonde observations is also challenging because radiosonde stations are sparse, their trends are likely cold biased, and the bias magnitude cannot be accurately determined (e.g., Sherwood et al. 2005; Randel and Wu 2006; Titchner et al. 2009). Discontinuities in the radiosonde data may propagate into reanalysis trends (Zhou et al., 2021), which ingests available data into a model to produce a physically coherent gridded product. Furthermore, the amplification ratio (e.g., the ratio of 700 mb and 250 mb warming) is also dataset dependent and has spread among the CMIP6 models (Keil et al., 2021).

In part due to such significant observational uncertainties, the consistency of modeled-versus-observed amplification is dependent on the amplification metric, dataset (and dataset version), and time period considered (Fu et al. 2011; Po-Chedley et al. 2012; Seidel et al. 2012; Santer et al. 2017a; Keil et al. 2021; Zhou et al., 2021). It is likely that differences

between models and observations are at least partially due to observational biases in upper air observations (Po-Chedley et al. 2015; Sherwood et al. 2015).

Independent analyses of high quality temperature datasets derived from global position system (GPS) radio occultation (RO) measurements show upper tropospheric warming and vertical amplification that is consistent with climate models (Fig. 1; Steiner et al. 2011; 2020; Vergados et al. 2021; Ladstädter et al. 2023). Other independent lines of evidence that suggest the tropics follow a near moist adiabatic profile come from thermal wind balance (Allen and Sherwood 2008; Sherwood et al. 2015) and from Johnson and Xie (2010), who argue from the distribution of convection in the tropics that there is no systematic trend toward large-scale destabilization of the temperature profile. Furthermore, underappreciated structural uncertainty in surface temperature trends likely support a wider range of vertical amplification values (Menemenlis et al. 2025) than has been recognized in previous analyses. As in Section 5.3, when comparing models and observations, the DOE CWG Report fails to account for observational biases and uncertainty.

To correct this issue, the DOE CWG Report must include information about observational uncertainty when comparing models and observations and include this information in figures. The DOE CWG Report must also compare climate models to upper tropospheric temperature trends derived from GPS RO data, given the stability of GPS RO derived temperature observations.

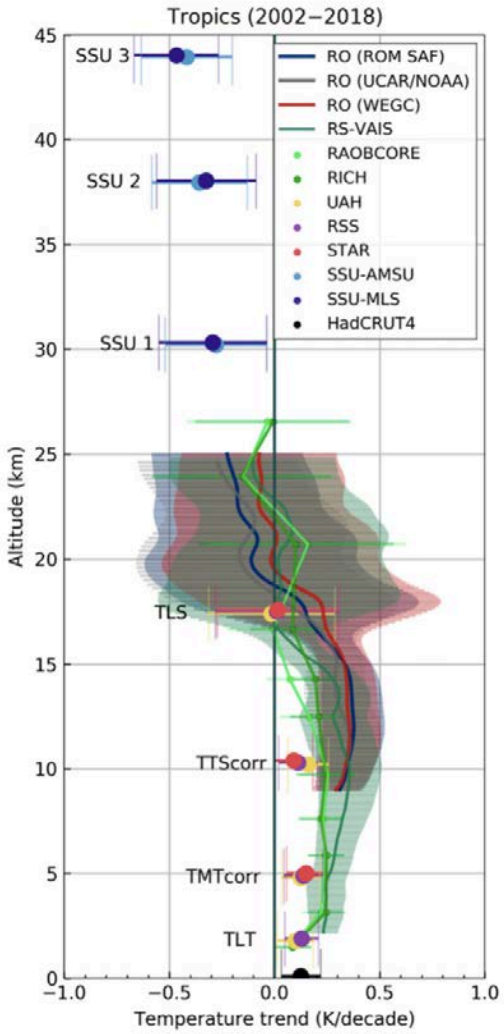

Fig. 1. Upper-air temperature trends 2002–18 from different observations for the tropics (20ºS–20ºN). Layer-average temperature trends are shown for MSU-AMSU (RSS, STAR, UAH) and for merged SSU records. Vertically resolved trends are shown for radiosonde records (RS-VAIS, RICH, RAOBCORE) and for RO records (ROM SAF, UCAR/NOAA, WEGC). Surface temperature trends from HadCRUT4 are also indicated. Trends were computed with multiple linear regression. The uncertainty of trends is indicated at the 95% confidence level. Reproduced from Steiner et al. (2020).

Page 37 and 38, Technical Comment

The statement, “The atmosphere’s temperature profile is a case where models are not merely uncertain but also show a common warming bias relative to observations. This suggests that they misrepresent certain fundamental feedback processes. The IPCC AR6 did not assess this issue,” is incorrect. Models and observations were compared in Chapter 3.3.1.2.1 of the IPCC AR6 Report (Eyring et al. 2021). The report shows that tropical atmospheric temperature trends from radiosonde observations and models are in agreement over 1979 to 1997, but diverge over 1998 to 2014 with observations showing weaker tropospheric warming compared to climate models. This latter time period corresponds to the so-called “hiatus” period in which internal

climate variability appears to have reduced surface warming (Dai et al. 2015). The pattern of warming, which can be modulated substantially by internal variability, affects surface warming in regions of deep atmospheric convection which is an important control on vertical amplification behavior (Flannaghan et al. 2014; Tuel, 2019; Po-Chedley et al. 2021). When models are forced to match the spatio-temporal pattern of real-world surface variability and trends via prescribed sea surface temperatures, the simulated vertical profile of atmospheric temperature trends over this sub-period and the full satellite era are in better accord with observations (Mitchell et al. 2020; Po-Chedley et al., 2021). This suggests that some of this model-observational discrepancy may be due to internal variability. As noted in the Mitchell et al. (2020) study:

> As for the modeled tropospheric trends, however, the discrepancy with observations is much larger after 1998. We suspect that one cause of this discrepancy is related to the fact that the 1998-2014 period corresponds to the occurrence of the so called 'global warming hiatus' (see Fyfe *et al* 2016, for a recent update of this debate). If the hiatus is indeed related to an increased heat uptake by the oceans, as suggested by some studies (Meehl *et al* 2011, Meehl *et al* 2014), it cannot be considered an externally forced process: therefore, one would not expect it to be captured in the models over the same time period. Another contributing factor is that 1997/1998 had one of the largest El Niño events on record, which, given the short period the trend is calculated over, becomes important. Indeed, if the analysis is repeated for the 1999-2014 period (i.e. missing the large El Niño year), the tropospheric observational trends are higher, and in better agreement with the coupled model estimates (figure S3). Be that as it may, we note here for the record that from 1998 to 2014, the CMIP5 models warm, on average 4 to 5 times faster than the observations, and in one model the warming is 10 times larger than the observations.

In addition to the effect of internal variability, biases in the forcings prescribed to models may also affect model-observational agreement (as discussed in the comment on Section 5.3 of the DOE CWG Report). The relative role of observational biases, model biases, internal variability, and model forcing biases in model-observation differences in the vertical profile of atmospheric temperature trends is not known. Discussion of the myriad of factors that affect the evaluation of the vertical structure of simulated temperature trends is omitted from the DOE CWG Report.

To correct this issue:

- **The DOE CWG should remove the assertion that the IPCC Report did not discuss this issue.**
- **The authors should include discussion of the potential role of internal variability and potential model forcing biases in explaining model-versus-observed warming differences.**
- **The DOE CWG Report should include the spread in trends across large model ensembles to account for the effects of internal climate variability (e.g., in DOE CWG Figure 5.6).**

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Section 5.5: Stratospheric Cooling

Stephen Po-Chedley[1], Benjamin Santer[2], and Qiang Fu[1]
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Summary: This section of the DOE CWG Report inaccurately and incompletely refers to published literature, makes inaccurate claims, and ignores the strong observed and simulated cooling of the middle and upper stratosphere. The DOE CWG Report asserts that observed lower stratospheric warming since the year 2000 is at odds with model simulations. This claim is incorrect.

The following changes must be made to Section 5.5 of the DOE CWG Report:

- **The authors must describe the strong middle and upper stratospheric cooling and that this cooling strongly improves the detectability of anthropogenic influence on climate. The authors must not compare vertical fingerprinting results over the full satellite era to trends in individual atmospheric layers in sub-selected time intervals.**
- **The authors should include the omitted text in their block quotation of Philipona et al. 2018.**
- **The authors must accurately describe atmospheric temperature change in different layers. The middle and upper stratosphere cool throughout the satellite era (1979 to present) and the lower stratosphere cools over 1979 to 2000 and then experiences muted trends after the year 2000.**

Page 38, Technical Comment

In framing this section of the DOE CWG Report, it is stated that the expected “fingerprint” of anthropogenic climate change is tropospheric warming and stratospheric cooling. While this is true for fingerprints derived over the full satellite era, the exact details of the vertical fingerprint pattern would vary in the *lower* stratosphere if other time periods were used. This is for the reasons stated in the DOE CWG Report: that stratospheric temperature change is “influenced by ozone depletion and recovery.”

It is important to note that because of the vertical distribution of ozone, the effects of ozone concentration changes are strongest in the *lower* stratosphere; in the middle and upper stratosphere, cooling is expected both in the ozone depletion and ozone recovery period. This is exactly what is observed: stratospheric cooling in the middle and upper stratosphere over the full observed time period as well as over the ozone depletion and recovery periods (see trends and time series in SSU2 and SSU3 in Santer et al. 2023, 2025; Maycock et al. 2018). Regardless, anthropogenic influence on atmospheric temperature is detectable over the satellite

era and is substantially stronger when information from the middle and upper stratosphere is included (Santer et al. 2023).

To correct this issue, the authors must not compare vertical fingerprinting results over the full satellite era to trends in individual atmospheric layers in sub-selected time intervals. The authors must describe the strong observed middle and upper stratospheric cooling and that this cooling strongly improves the detectability of anthropogenic influence on climate.

Page 38, Technical Comment

The study cited by the DOE CWG Report in their discussion of lower stratospheric temperature change (Philipona et al. 2018) derived lower stratospheric warming from sparse radiosonde observations, but also showed that satellite observations over the same post-2000 time period show weak cooling, consistent with observational results in other studies (Santer et al. 2021). Regardless of whether the observed trend is weakly positive or negative, the key point here is that model-simulated lower stratospheric temperature trends after the year 2000 are not at odds with observational results (Santer et al. 2021; 2023). The model post-2000 trends encompass both cooling and warming, consistent with a weak forced trend arising from the offsetting effects of warming from ozone recovery and CO_2-induced lower stratospheric cooling. The DOE CWG Report is incorrect in stating that "the stratosphere has shown some warming since [2000], contrary to model projections," since the sign of the observed trend is not well constrained and because model simulations produce both positive and negative lower stratospheric temperature trends for the post-2000 period.

Page 39, Reference Comment

In the block quote referencing Philipona et al. (2018), the authors incorrectly omit part of the last sentence, which explains the observed behavior (bolded here): "Trend estimates are somewhat sensitive to homogeneity assessment choices, but all investigated radiosonde data sets suggest a change from late twentieth century cooling to early 21st century warming in the lower stratosphere**, which is consistent with a reversal from ozone depletion to recovery from the effects of ozone-depleting substances**."

To correct this error, the authors must include the full quotation.

Page 39, Technical Comment

The DOE CWG Report makes a stand-alone statement that "Santer et al. (2023) use updated data to show that a cooling trend has not re-emerged in the lower stratosphere." This statement is incorrect. Santer et al. (2023) show that expected stratospheric cooling in the middle and upper stratosphere is evident in both climate models and observations, both before and after 2000. While Santer et al. (2023) do not show the observed time evolution of lower stratospheric temperature trends over different time periods, that information was provided in an earlier paper

(Santer et al. 2021, their Fig. 4). Santer et al. (2021) showed that in both the observations and model simulations, trends in lower stratospheric temperature differ markedly between the pre-2000 ozone depletion and post-2000 ozone recovery periods. Cooling is large over the depletion period and muted over the recovery period. This common behavior in simulations and observations is due to the competing effects of ozone concentration changes and increases in CO_2. The DOE Report is silent on the fact that observational evidence of strong, persistent middle and upper stratospheric cooling and tropospheric warming is consistent with model simulations and with a "fingerprint" of anthropogenic influence (Santer et al. 2023, 2025; Maycock et al. 2018).

To correct this issue, the authors must provide an accurate description of stratospheric temperature change by layer: In the lower stratosphere, observed stratospheric cooling over 1979 - 2000 is followed by muted trends after the year 2000. In the middle and upper stratosphere, strong cooling is evident throughout the satellite era (1979 to present). In both cases, observed stratospheric changes are within the range of model results given observational uncertainties.

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Comments on Section 5.6 “Snow cover mismatch”

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The DOE report in Section 5.6 makes erroneous and misleading claims about Northern Hemisphere and West Pacific snow extent and snow melt trends.

1. Suggestion that climate models have failed to simulate observed trends in wintertime Northern Hemisphere snow cover, but use only one observational product and do not properly convey that it has known trend biases.

2. Ignore more robust snow cover declines during other times of the year that are also important for water resources.

3. Incorrect suggestions that there has been minimal snowpack changes in Pacific states.

To correct these technical flaws, the following changes much be made in the DOE CWG report document:

1) Update the text to correctly cite the relevant scientific literature and ensure it is known that snow cover extent (SCE) is declining in the N. Hemisphere in most months, and the balance of evidence suggests that this is true in all months.
2) Discuss the role of boreal spring declines in snow cover and decreases in Western US snowpack for water resources.
3) Remove erroneous statements about trends in specific states or the hemisphere as a whole.

The DOE report is focused heavily on the ability of climate models to reproduce certain observations and/or observed sensitivities (for instance, climate variables such as snow cover and/or water equivalent to temperature). Section 5.6 of the DOE CWG report has two main points: first, highlighting observational evidence indicating the lack of decline in Northern Hemisphere snow cover extent in contrast to decreasing trends simulated by climate models, and second, observational evidence for changes in winter snow accumulation in the coastal Western U.S. We address here the observational evidence in these two areas, which we believe has been

inadequately addressed in the report. We also stress that no single thermal or physical indicator is a sufficient metric for characterizing the response to a warming climate.

Northern Hemisphere Snow Cover Extent (SCE):

The report notes that the Rutgers SCE product for the Northern Hemisphere shows no statistically significant trend in winter, whereas climate models generally predict declines. We first note that the observational product discussed in the DOE report (the Rutgers/NOAA Climate Data Record) is not robust with respect to NH winter trends and disagrees with many other observational datasets (Figure 1 below. See Brown and Derksen, 2013; Hori et al. 2017; Mudryk et al., 2017; Urraca and Gobron, 2023). Mudryk et al. (2020) analyzed six observation-based snow extent products for the period 1981–2018 and showed that trends in NH SCE are negative in all months and strongly linked to surface temperature. Other important metrics such as snow mass have also declined, especially over North America (Pulliainen et al., 2020; Gottlieb and Mankin, 2024). Although the authors of the DOE CWG report acknowledge the observational uncertainty, this is a known issue that is simply brushed aside to conclude, "Further work to reconcile conflicting trends in observational data sets is required." This presents a misleading balance of evidence in the overarching goal discussing the manner in which "greenhouse gas emissions have affected, or will affect, the Nation's climate, extreme weather events, and selected metrics of societal well-being" (their first sentence of the Executive summary).

Furthermore, the climatology of SCE in winter has improved in CMIP6 compared to CMIP5, and despite the structural uncertainties in the observations, the models appear to be consistent with the observed trends (Mudryk et al. 2020).

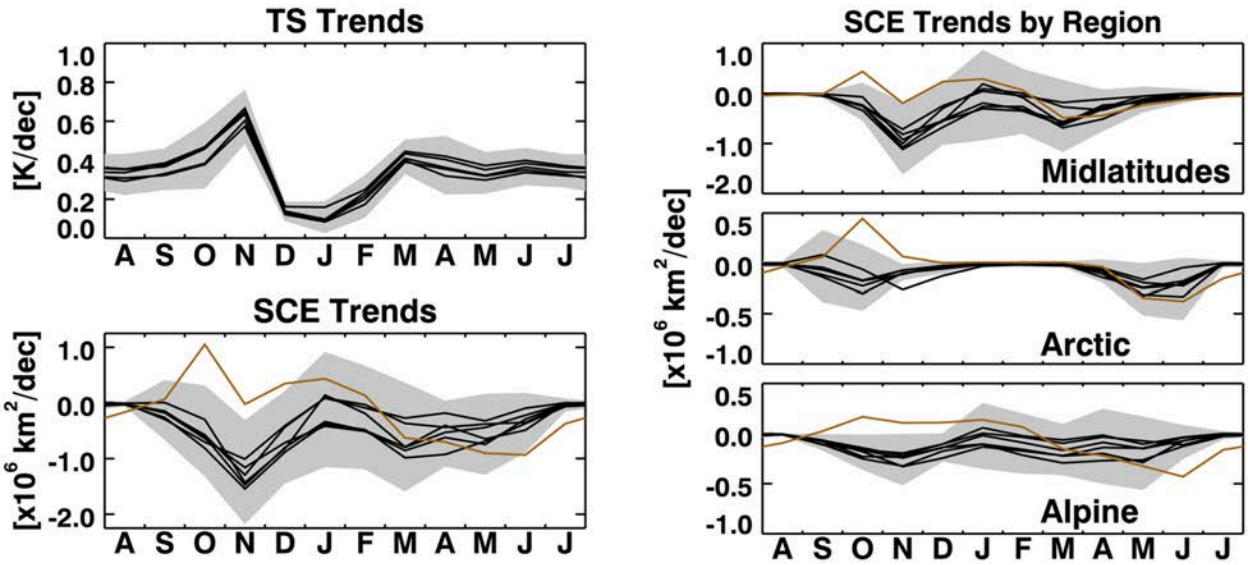

Figure 1. Adopted from Mudryk et al. (2017). Estimates of observed monthly Northern Hemisphere (NH) (top left) surface temperature (TS) trends from 1981-2010 and (bottom left)

snow cover extent (SCE) trends. SCE trends are also shown for (right column) three subregions. Trends calculated from the NOAA Climate Data Record (NCDR) are shown in brown.

The strong downward trends in SCE in spring are also very important to ecosystems, hydrology, water security, and agriculture (Mankin et al., 2015; Qin et al., 2020). A substantial fraction of the world's population relies on seasonal melt (Barnett et al., 2005). From a physical climate perspective, the snow trends in spring are important simply because downward solar radiation is much higher in spring than winter, hence the contrast in albedo (snow cover vs snow free) makes more difference to net radiation at the land surface (Déry and Brown, 2007), and to related variables like evapotranspiration and photosynthesis.

Spring snowpack declines have been observed in many mid-latitude basins and these declines have been attributed to human influence (Paik and Min, 2020; Gottlieb and Mankin, 2024). For example, Milly and Dunne (2020) showed that ongoing downward trends (of roughly 10% per degree C) in the annual discharge of the Colorado River are traceable to changes in spring SCE in the river's headwater regions that lead to increased net radiation and evapotranspiration, and decreased river discharge. The Colorado River is the major water supply source for over 40 million people in the U.S, Southwest, and several million acres of irrigated agriculture.

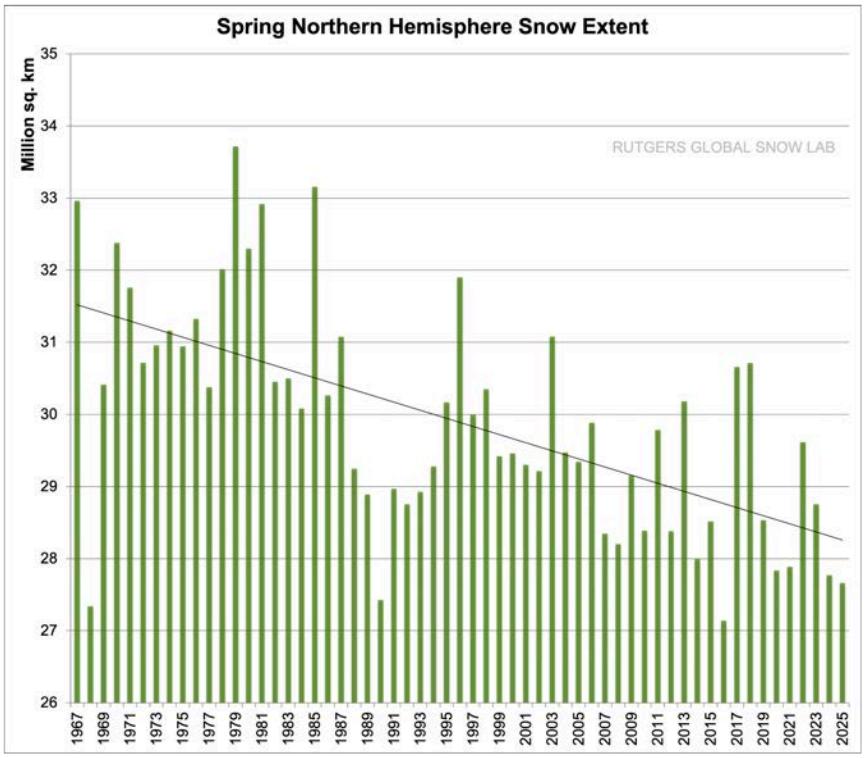

Figure 2: Spring trends in N. Hemisphere SCE from https://climate.rutgers.edu/snowcover/chart_seasonal.php?ui_set=nhland&ui_season=2 (accessed Aug 18, 2025)

We note that the authors of the DOE CWG report chose to only report on wintertime snow trends and did not show data from other seasons. Even the Rutgers data they cite show substantial springtime snow cover declines (Figure 2 below) and these anomalies should be included in the broader context of a changing cryosphere.

Pacific Coast snow water storage: Since the work of Church on Mt. Rose and elsewhere in the West in the 1920s and 1930s, hydrologists have recognized the importance of snow water equivalent (SWE) as an indicator of spring and summer streamflow from snow-dominated watersheds in the Western U.S. By the 1940s, measurements of SWE (typically using the Mt. Rose snow sampler, which remains the standard for manual observations of SWE) were being used routinely to forecast seasonal streamflow in the major rivers of the Western U.S. for water supply purposes. That function is shared (depending on location and purpose of the forecasts) by the USDA Natural Resources Conservation Service and the National Weather Service River Forecast Centers.

Numerous papers (e.g, Mote et al. 2005, Luce et al. 2014, Mote et al. 2018; Musselman et al., 2021; Hale et al., 2023) have documented declines in western snowpack, especially using the most common observation date of April 1, and many papers (those just listed, plus Pierce et al. 2008) have statistically linked these declines to warming. The first analysis of trends in snowpack in both the western US and southwestern Canada analyzed the SWE observation archive from NRCS, California's Department of Water Resources, as well as Canadian agencies (suitably pre-processed for error removal) for long-term trends (Mote et al., 2005). They found pervasive downward trends in SWE for the period 1950-97 which were most pronounced at locations with relatively warm (close to 0°C) average winter temperatures, and corroborated these results with an independent calculation using a hydrology model. A subsequent paper (Mote et al., 2018) updated the observational records through 2016, and reached essentially the same conclusion using a range of starting dates for analysis, and in addition showed that the declines were observed in every month.

Additional, independent evidence for declines in spring snowpack are provided by streamflow observations. Stewart et al. (2005) independently analyzed streamflow at unregulated stream gauges across the West, and found that for watersheds where snow contributed much of the annual runoff, there had been persistent shifts in runoff earlier in the year (more in winter, less in spring and summer) over a period of record comparable to that used by Mote et al. (2005). Stewart et al. also analyzed streamflow timing in unregulated gauges with minimal snow effect, and found that they did not exhibit the same timing shifts. Taken together, these two studies offer strong evidence that ongoing warming across the Western U.S. (Harpold et al., 2017) has led to declining spring snow water storage and associated shifts in seasonal runoff timing.

These documented changes are critical to water managers, as manmade reservoir storage (which is mostly intended to shift water availability from spring and early summer to the period of maximum water demand later in the year) has historically been augmented by natural storage in

the snowpack (SWE being the key indicator). That natural storage is being eroded as indicated by declining spring SWE.

The report ignores all of the above work. It is worth noting that the reality of warming-driven snow droughts is widely accepted by water managers in the US, who are therefore not well served by the very incomplete picture presented in section 5.6. The treatment of western snowpack changes rests solely on a less relevant set of results with a different variable. It states "A comprehensive reconstruction of snowfall for the main source regions (Cascades and Sierra Nevada Mountains) indicates no significant trends in annual totals since the late 19th century (Christy 2022)". Presumably the authors meant to include a 2012 paper by Christy which covers California (the 2022 paper uses the same methods as 2012 applied to Washington and Oregon).

Much more important however is that both papers are based on snowfall measurements obtained from records at stations where manual measurements (essentially with rulers) of snow depth are made. The variable examined by the authors is snowfall accumulated each day over the winter and spring. This is not SWE, and in fact is a poor indicator of SWE (which as indicated above is a widely used index of runoff) because a) the stations used in both papers are heavily biased towards low elevations, and are poor indicators of conditions at higher elevations where SWE is largest; and b) because they are at low, relatively warm elevations, some stations transition from snow covered to bare throughout the winter, hence the total accumulated winter snow depth is a poor indicator of spring conditions.

Notably, the authors of the Christy reference cited in the report acknowledge that SWE is a better indicator of snowpack water availability. Taken together, it's not surprising that the two Christy papers found an absence of trends for both California (2012) and Washington and Oregon (2022), but what of relevance is to be taken from those results relevant to the DOE report isn't clear. The alternative papers cited above in this comment, which document considerable declines in mountain SWE, suggest there are significant vulnerabilities with respect to climate warming, snowpack, and water resources in the Western US which warrant acknowledgement in the DOE report.

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Section 5.7: Hemispheric symmetry of the planetary albedo

edited by A.E. Dessler, Texas A&M University

Section 5.7 of the DOE CWG report explains that Earth's Northern and Southern Hemispheres have nearly identical albedos, despite big land-ocean differences, likely due to cloud compensation in the south. Observations show this symmetry within ~0.1 W/m², but most CMIP6 models fail to reproduce it, with errors up to 5 W/m². This mismatch suggests problems with cloud feedbacks and reduces confidence in some future climate projections

This is a technical comment on Section 5.7 (p. 41-42)

The authors of the review rightly call out Earth's Energy budget (EEB) as being an important measure of climate as Earth's Energy Imbalance (EEI) is among the fundamental drivers of climate change. Fundamental insights have been provided into EEB by the Clouds and the Earth's Radiant Energy System (CERES) project since 2000. The authors are also quick to point out an intriguing aspect of EEB in CERES is the symmetry of albedo between hemispheres, as discussed in Stephens et al. (2015).

The Earth represents an extraordinarily complex system with extensive observational datasets. When analyzing such vast amounts of data, it is inevitable that patterns will emerge that appear anomalous or that current models struggle to reproduce. However, the mere existence of such patterns does not automatically establish their fundamental importance to climate science. Without clear mechanistic understanding, demonstrated persistence across extended timescales, or quantifiable impacts on climate-relevant parameters, attributing significance to these observations requires substantial additional evidence and analysis.

That is the case here: Beyond the mere existence of the symmetry, any broader significance is unproven. We don't know if the symmetry holds across longer time periods nor do we know whether a few W/m2 error (out of O(1,000) W/m2 of solar insolation) will translate into errors in parameters we care about (see Stephens et al. 2022 and Hakuba et al. 2024). We don't know if it is indicative of a feedback nor do we know whether, if so, if it might reduce or exacerbate climate change. For these and various other reasons (see below), there is little justification for viewing the symmetry as a fundamental parameter of our climate system.

The DOE CWG report nonetheless treats this symmetry as a fundamental climate parameter despite the lack of supporting evidence. This assumption is baseless and a major problem with this section.

To correct this error, the following additions must be made in the DOE CWG document:

- An acknowledgment should be added that the observed symmetry is not a key parameter in our climate system.
- A discussion of the peer-reviewed literature should be included that discusses how the albedo symmetry connects to changes in climate sensitivity, cloud feedback, or future warming.

Further, it is a major error of omission to focus on the albedo symmetry in the CERES data rather than the more important quantity, EEI, which is increasing over time and with a rate that is stronger than simulated by most climate models. (Raghuraman et al. 2021). This measurement by CERES reveals increases in the accumulation of heat by the climate system that are likely to result in increasingly rapid global warming. It also suggests stronger integrated ocean warming, ice sheet melt, and sea level rise, and potentially stronger feedbacks than are simulated by modern climate models.

To correct this error, the following additions must be made in the DOE CWG document:

- Incorporation of a broader discussion on EEI, its connections to ocean warming, ice sheet melt, and sea level rise.
- Addition of a discussion of uncertainty in feedbacks and the approaches necessary for better constraining them, including prolonged monitoring of EEI and the reservoirs of heat in the Earth system through projects such as CERES.

Despite citing their work in Fig 5.8, the authors selectively ignore key statements made by Rugenstein and Hakuba (2023) based on their analysis including that the albedo symmetry is a function of the current climate state (i.e. not intrinsic), and that albedo will likely evolve to be asymmetric in coming decades. Also, the authors show in their Fig. 5.8, yet fail to mention in the text, that some models do simulate a symmetry in albedo between the hemispheres within measurement uncertainties - including the high sensitivity E3SM1.0. They further fail to mention that these models do not systematically differ in their projections from models that fail to capture the symmetry. This observation suggests, consistent with the findings of Rugenstein and Hakuba (2023) that the fidelity of future projections does not depend on a model's ability to simulate the symmetry and that is not indicative of a mechanism of climate stability.

To correct this error, the following additions must be made in the DOE CWG document:

- an acknowledgment that our best understanding of the asymmetry is that it is likely not constant but variable in time
- an acknowledgment that some models do simulate the observed symmetry in albedo between hemispheres, including some with very high climate sensitivity
- an acknowledgment that the ability of a climate model to reproduce the observed symmetry is not indicative of the climate projections it generates

The mechanisms that would underpin any "cloud feedback processes" suggested by the authors would rely on fields governed by temperature and moisture such as tropospheric stability, upper-level dryness, boundary layer inversion strength and related fields. Yet temperature and moisture are governed by the total radiative flux, not the shortwave portion of the spectrum. Longwave fluxes and hence net fluxes do not exhibit hemispheric symmetry. This calls into question the existence of any regulation mechanism.

To correct this error, the following changes must be made in the DOE CWG document:

- An acknowledgement that confidence in inferring errors in projected climate as a result of a given model bias require, at a minimum, a robust identification of model biases, a demonstration that the degree of bias in a given model is tied to the character of its projections, and a mechanism that connects the present-day bias to simulated climate feedbacks.

References:

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Response to section 5.8

A.E. Dessler, Texas A&M University

This section is a **technical comment** on Section 5.8 of the DOE CWG report, beginning on **page 42**, and Figure 5.9, which is on **page 43**.

In this section of the DOE report, the authors write: “One of the largest discrepancies between models and observations is in the U.S. Corn Belt, a region of particular importance to global food production. Figure 5.9 shows the warming trends for summertime (June, July, August) for the 12-state Corn Belt (IN, IA, IL, ND, SD, MO, MN, WI, MI, OH, KS, NE) during 1973-2022. All 36 climate models (red) warm far too rapidly compared to observations (blue).” This is part of an overarching argument that models are not good enough to use for prediction of the future climate.

As described below, this argument relies on sloppy data analysis and cherry picking to reach a conclusion that analysis of the full data record does not support.

Response:

First, climate models have an admirable track record of predicting the *global average* temperature (Hausfather et al., 2020). While the global average temperature is well constrained by planetary energy balance, how that warming is distributed around the planet can vary greatly due to what we refer to as *unforced variability*. This phenomenon is similar to the randomness we observe in weather patterns, but are driven by the ocean circulation on much longer time scales, such as the El Niño/La Niña oscillations.

As an example, you can take a climate model and run it many times, with identical emissions of carbon dioxide, but with each run starting with a slightly different state of the climate system. For these runs, the global average temperature change is similar, but how the warming is distributed varies greatly:

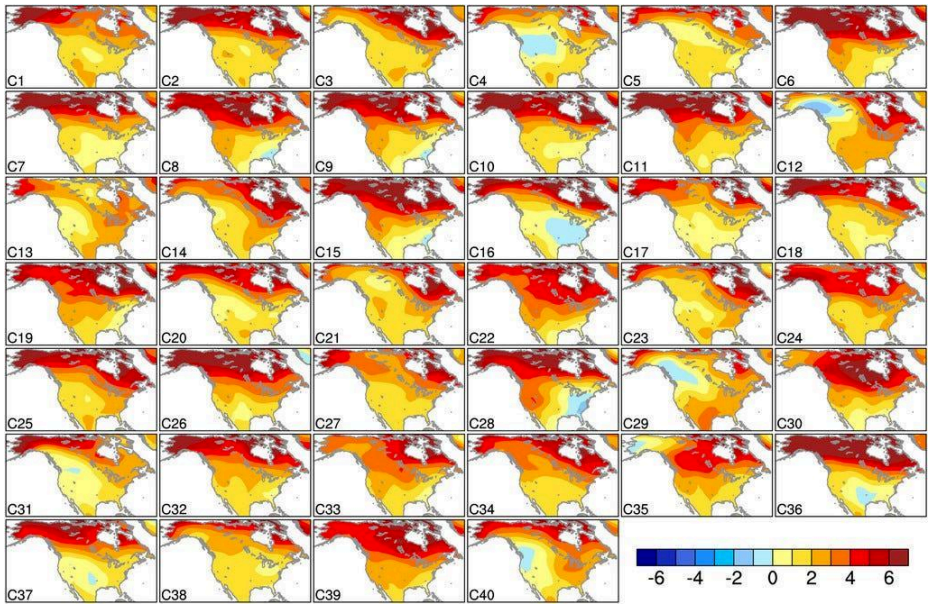

Figure 1. Winter surface temperature trends [2010–60; °C (51 yr)$^{-1}$] from each of the 40 CCSM3 ensemble members. From Fig. 1 of Deser et al. (2014)

In some of the ensemble members (e.g., C16, C28), the Southeast U.S. cools between 2010 and 2060, while in most others it warms. In some members (e.g., C4, C29, C40), the Pacific Northwest cools, while in most others it warms.

This has important implications for the plot in Sect. 5.8. To see this, let's take a single model, in this case, ACCESS-ESM1-5, which has an ensemble of 40 historical runs in the CMIP6 archive. Each run has different initial conditions, so each run of the model will have a different pattern of warming and cooling, just like we see in the plot above.

Summertime central U.S. warming in this one model ensemble looks like this:

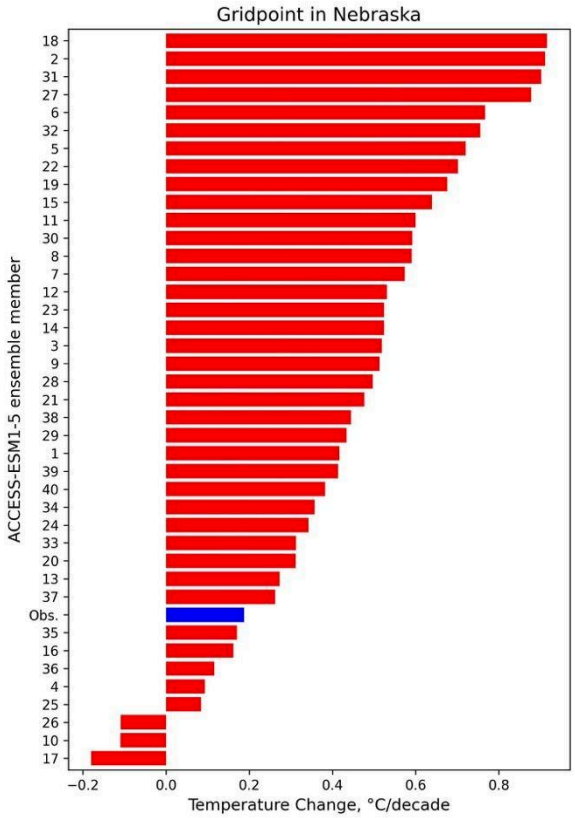

Figure 2. Warming of a grid point in Nebraska between 1970 and 2014 from 40 runs of a single model (ACCESS-ESM1-5) with different initial conditions. Model results are red, the observation is in blue.

This shows the importance of unforced variability: 20% of the runs show smaller warming than observations, and 8% show *cooling*. At the other end, there are ensemble members that show five times the observed warming. This spread is due entirely to different initial conditions, so this is an example of chaos in a strongly non-linear system.

So is the model in Fig. 2 (ACCESS-ESM1-5) overestimating warming in this region or not? *There's no way to know*. If you pick ensemble member 17, the model is underestimating the warming. If you pick ensemble member 35, the model is doing a fantastic job, while ensemble member 18 is overestimating the warming.

Without figuring out which of these ensemble members has unforced variability matching the unforced variability in the real world (this is really hard, by the way), the only correct conclusion is that the observed warming falls within the envelope of warming predicted by this model, so you **cannot** conclude that the ACCESS-ESM1-5 is overestimating warming.

This also means that Figure 5.9 of the DOE report, which shows one value for this model, ~0.6°C, is scientifically inaccurate.

To construct Fig. 5.9 of the report, I'm guessing a random run of each model was selected. So let's remake the plot, but using the ensemble member from each model that shows the *lowest warming* in this region. I limit myself to models that have ensembles with at least five runs in the CMIP6 archive.

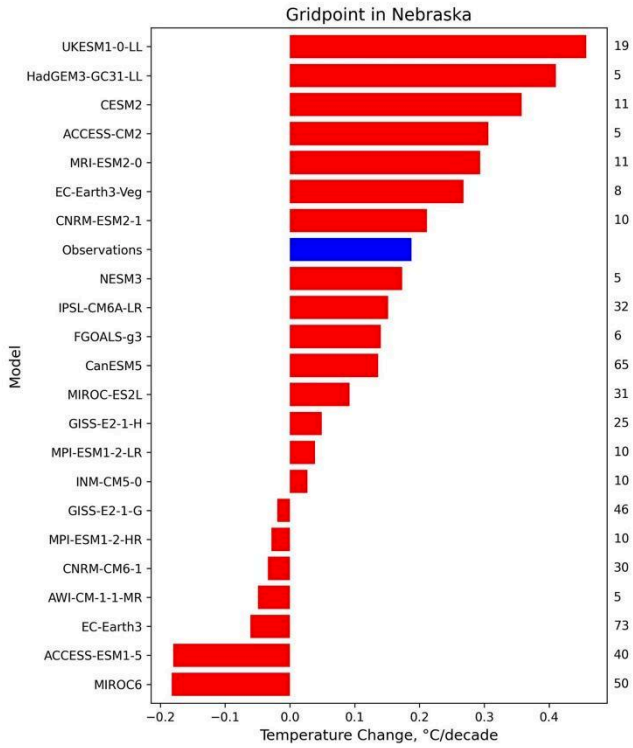

Figure 3. Warming of a grid point in Nebraska between 1970 and 2014 from the ensemble member of each model with the lowest warming in this region. Model results are red, the observation is in blue. The number of ensemble members for each model is displayed off the right edge of the plot.

Figure 3 shows that the results of the ACCESS-ESM1-5 are generalizable. Two thirds of the CMIP6 models produce warming lower than observations in their ensembles. Again, this doesn't prove the models are right, but it does say that **naive comparisons that ignore unforced variability, like Fig. 5.9 in the DOE report, are biased and unreliable**.

What's a better way to do the calculation?

While unforced variability can impact any single location, if you look globally, the impact of it cancels out — regions that are cooled by unforced variability are cancelled by other regions that are warmed. Thus, the right way to do this calculation is globally.

To that end, I've taken a single run of each of 45 CMIP6 historical model runs and the observations and calculated this same change in summertime surface temperature, but at each point on the globe.

If the CMIP6 models are accurately simulating the climate, I would expect that areas where the model average is warming faster than the observations would be about equal to the area where the model average is warming slower.

If I screen models by eliminating those (Hausfather et al., 2022) that have equilibrium climate sensitivity that's out of the accepted range (2°C-4°C), then the average warming of the models exceeds observations over 48% of the area of the Earth, with observed warming exceeding the models over the other 52%. [If I don't remove the too-hot models, then 63% of the globe shows models warming more than observations]

These results are exactly what we would expect if the models are accurately simulating the climate system.

What's really going on?

Fig. 4 shows the spatial pattern of the difference between the average of the screened model ensemble and the observations. Red colors indicate where the average model warming is faster than observations while blue areas show that models are warming slower. As mentioned above, 52% of the area of the globe is blue, indicating places where the models are warming slower than observations.

The box over North America shows the region that the DOE plot focuses on. It's clear that the region that was selected in the DOE plot is *the place in the Northern Hemisphere where models look the absolute worst.* This constitutes a textbook case of cherry picking — the practice of selectively extracting a narrow subset of data that contradicts the broader dataset to support a predetermined conclusion.

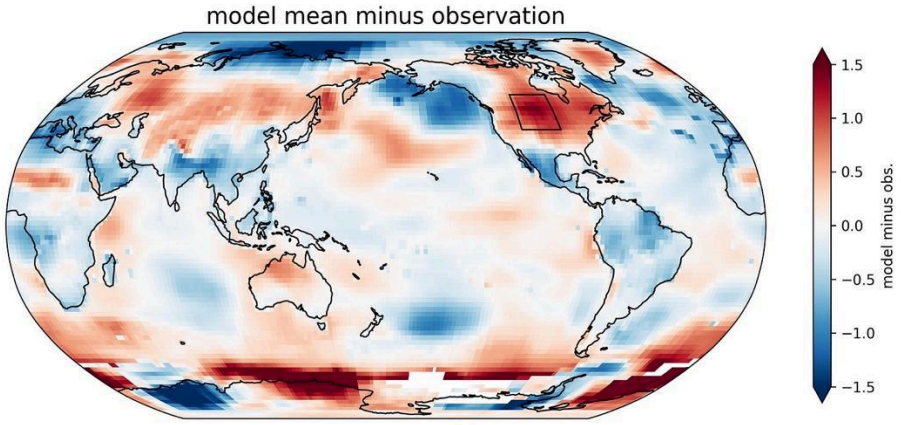

Figure 4. Average warming in the screened models minus observed warming, 1970-2023. the box indicates the region that Fig. 5.9 in the DOE report focuses on. red regions indicate that the models are warming faster than observations, blue indicates the opposite.

I believe the author of Figure 5.9 of the DOE report is Dr. Roy Spencer. I should note that I have criticized Spencer for this type of cherry picking (Dessler, 2011):

> First, [Spencer and Braswell] analyzed 14 models, but they plotted only six models and the particular observational data set that provided maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion.

This region is special

But this leads to the question of why models do so poorly in this one particular region. It turns out that this is a special region because of intensive agriculture there (e.g., Mueller et al., 2016; Alter et al., 2018). This is the most agriculturally productive region on the planet, and land-use changes over the last few decades have largely offset greenhouse gas warming here.

To the extent that models have problems in this very small region, it's connected to how well the models are handling the specific land-use changes occurring only in this region, not how well they represent global warming.

Conclusions

Climate models do an excellent job of simulating the global climate. This section focuses on one region (making up less than 1% of the area of the planet) where the models don't do as well as in other regions. The analysis ignores the impact of unforced variability, which makes the models look worse than they actually are. We also understand that this is a unique region of the planet and how models do here tells us little about how well the models do globally.

To fix this, the authors must:

- Acknowledge that they selected the region where the models do worst
- Acknowledge that global analyses shows that the models do quite well at simulating the magnitude of historical warming
- Improve this analysis using a multi-ensemble approach that demonstrates that the models do better than the plot that's presently in the report
- Explain why models do poorly here (due to intensive agriculture in this region); explain how this does not apply anywhere else on the globe

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Hausfather, Z., Marvel, K., Schmid, G. A., Nielsen-Gammon, J. W., & Zelinka, M. (2022). Climate simulations: recognize the 'hot model' problem. *Nature, 605*, doi:https://www.nature.com/articles/d41586-022-01192-2

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Code to reproduce the plots: https://github.com/aedessler/TCB_Trump_Graphic

Comment on the DOE CWG report, Section 6.2: Hurricanes and Tropical Cyclones

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Pages: 46, Section 6.2, Hurricanes and Tropical Cyclones (Executive Summary)

Comment type: Technical

Comment:

The Executive Summary states:

"Extreme convective storms, hurricanes, tornadoes, floods and droughts exhibit considerable natural variability, but long-term increases are not detected."

In regards to hurricanes, this statement is true only for overall frequency.The key research findings on observed changes in hurricane hazards have been omitted, which is that hurricanes are becoming more hazardous, reaching higher intensity, intensifying more quickly, and producing more rainfall, which is increasing their inland impacts.

Pages: 48-52, Section 6.2, Hurricanes and Tropical Cyclones

Comment type: Technical

Comment:

This section of the DOE CWG Report begins by arguing that existing historical records of hurricanes and tropical cyclones (hereafter collectively called TCs) are generally too short or too flawed to detect plausible trends of such events. However, findings in the report rely almost entirely on analysis of such historical trends to reach conclusions that are only partially and selectively in agreement with those of the far more comprehensive discussions in IPCC AR6 and the broader scientific literature. Where the observational record of TCs fits a framework that suggests no change in the severity of TC impacts in recent years, the authors argue that the observational record proves that a warming climate is not causing TC hazards to worsen; no robust statistical analyses are offered

to support these statements. However, in cases where the broader scientific community has been able to perform robust statistical analyses of the observational record that suggest that TC impacts have worsened as the climate has warmed, the DOE CWG authors declare that the observational record is too short to assess any meaningful trends.

In general, Section 6.2 of the DOE CWG Report relies upon selectively chosen references, and contains a number of critical omissions that result in an incomplete presentation of the scientific community's knowledge regarding the ways in which TCs and their impacts are changed by a warming climate.

Critical omissions from Section 6.2 of the DOE CWG Report are listed below. These points will be elaborated and references are provided in the subsequent text:

- The DOE CWG Report neglects important evidence from theory and models reported in AR6 and in subsequent peer-reviewed literature.
- The DOE CWG Report stresses the finding that there are no statistically significant trends in, e.g., U.S. landfalling hurricane frequency, while not informing the reader that a) this outcome is in fact what is expected based on the current science and b) that plausible trends cannot easily be detected from a short historical record given the random noise and natural variability in the series.
- At the same time, Section 6.2 of the DOE CWG Report ignores more recent peer-reviewed literature as well as its own non-peer-reviewed data regarding hurricane intensity: there is an upward trend in the proportion of major tropical cyclones, a trend that *was* predicted.
- The DOE CWG Report does not directly address the IPCC AR6 findings that 1) tropical cyclone rainfall is already increasing and expected to continue to increase rapidly in the future (based on simple physics); and 2) TC intensification rates are already increasing and expected to continue to increase in the future (based on well-established dynamical theories). These are serious omissions, as flooding is the main source of mortality and damage in TCs, and increasing intensification rates will shorten the time window for preparations and evacuation.
- Although storm surge flooding has historically been one of the most damaging and deadly aspects of TCs, the authors do not address the scientific consensus that has shown that rising sea levels will amplify future storm surge flooding, and a recent study showing that extreme surges are already likely increasing along parts of the U.S. coastline.

Page: 48

Comment type: Technical

Comment:

The Report quotes from AR6:

AR6: "There is *low confidence* in most reported long-term (multidecadal to centennial) trends in TC frequency or intensity-based metrics due to changes in the technology used to collect the best-track data." (IPCC, 2021 p. 1585)

However, the next sentence in the IPCC AR6 report makes an important clarification:

"This should not be interpreted as implying that no physical (real) trends exist, but rather as indicating that either the quality or the temporal length of the data is not adequate to provide robust trend detection statements, particularly in the presence of multi-decadal variability."

This is a recurring issue in Section 6.2: the absence of statistically significant trends in the historical records does not mean that changes are not occurring. As will be outlined below, there are other good reasons to believe that changes in TC intensity and impacts are already occurring. Furthermore, there is no consensus prediction as to whether the number of TCs will increase or decrease (e.g., Knutson et al. 2020; Seneviratne et al. 2021); therefore, absence of a trend does not contradict the science.

Pages: 48

Comment type: Reference

Comment:

The following quote is attributed to IPCC, 2023 SPM, p. 9:

> "*It is likely that the global proportion of major (Category 3-5) tropical cyclone occurrence has increased over the last four decades … There is low confidence in long-term (multi-decadal to centennial) trends in the frequency of all-category tropical cyclones."*

The quoted text does not appear in the document identified as IPCC, 2023, SPM (full citation below as IPCC 2023). More likely, this is an edited and incorrectly cited quote from the 2021 IPCC AR6 Working Group 1 Summary for Policy Makers (cited below as IPCC 2021), which in full, reads:

"*It is likely that the global proportion of major (Category 3–5) tropical cyclone occurrence has increased over the last four decades, and it is very likely that the latitude where tropical cyclones in the western North Pacific reach their peak intensity has shifted northward; these changes cannot be explained by internal variability alone (medium confidence). There is low confidence in long-term (multi-decadal to centennial) trends in the frequency of all-category tropical cyclones. Event attribution studies and physical understanding indicate that human-induced climate change increases heavy precipitation associated with tropical cyclones (high confidence), but data limitations inhibit clear detection of past trends on the global scale."*

Page: 48 / Figure 6.2.1 (on page 49)

Comment type: Technical

From the Report:

"Figure 6.2.1 shows that on average, each year there are about 50 hurricanes, with about 25 reaching major hurricane status (Maue, 2025). There is substantial year-to-year and decadal variability, a weak decrease in the number of hurricanes, and a slight but insignificant increase in the number of major hurricanes. These two trends combine to create an overall increase in the proportion of major hurricanes.

Comment:

It is worth noting that this change in the proportion of major hurricanes, which is supported in AR6 and other published studies from the peer-reviewed literature (e.g., Seneviratne, et al., 2021; Kossin et al., 2020), is statistically significant (based on records from the standard historical database of TCs), even though the trends in number of hurricanes and major hurricanes are not statistically significant.

Page: 50

Comment type: Technical

From the Report:

"However, following 2005 there were no major hurricanes striking the U.S through 2016, the longest such period since 1920."

Comment:

This statement about the major hurricane drought is correct. However, there have also been 10 major U.S. hurricane landfalls in the 8 years after the drought (Harvey, Irma, Michael, Laura, Zeta, Ida, Ian, Idalia, Helene, and Milton), not including Maria in Puerto Rico, which would make it 11. These contrasts illustrate the weakness of using short records of infrequent events to identify important trends.

Page: 51

Comment type: Technical

From the report:

"...it is remarkable that the highest year in the entire record is 1886, with 7 hurricane landfalls, when human influences on the climate were much smaller than they are today."

Comment:

As noted above, there is no consensus prediction for trends in numbers of hurricanes, and especially not for landfalling hurricanes. Therefore the largest number occurring in 1886 is not in contradiction with AR6 or the published literature.

Page: 51

Comment type: Technical

From the Report:

"In summary, analyses of both global and regional variability and trends of hurricane activity provide the basis for detecting changes and understanding their causes."

Comment:

This statement is incomplete. Theory (Emanuel 1987; Ramsay and Sobel 2011; Emanuel 2017b) and modeling (Walsh et al. 2019; Knutson et al. 2020; Roberts et al. 2020, to name just a few of many papers) both provide reasons to believe that, in a warming climate, the fraction of major TCs will increase, and that TCs will get stronger, produce more rain, and intensify more rapidly. These findings support a statement from the IPCC AR6, cited by the authors of the DOE CWG: "...it is *likely* that the global proportion of major hurricanes (Categories 3-5) has increased in the last four decades." As noted by IPCC AR6, such changes cannot be explained by natural variability alone.

Page: 51

Comment type: Technical

From the Report:

“The relatively short historical record of hurricane activity, and the even shorter record from the satellite era, is not sufficient to assess whether recent hurricane activity is unusual relative to the background natural variability.”

Comment:

Similar to what is noted in the comment above for p. 48, Section 6.2 of the DOE CWG Report, the statement above should have added that the observational dataset cannot rule out plausible underlying trends in U.S. landfalling hurricanes, and that there is no consensus prediction that TC frequencies will change.

Page: 51 / Table 6.2.1

Comment type: Technical

From the Report:

Table 6.2.1 lists the estimated wind speed intensities of the 10 strongest hurricanes to make landfall in the U.S.

Comment:

By definition, landfalling hurricanes are extremely rare events, which means that an even longer historical record would be required to assess any plausible trend. Therefore, any trend, positive or negative, that might be inferred by some subjective standard (e.g., when the three most intense U.S. landfalling TCs occurred) cannot be considered statistically significant. Therefore, no conclusions, either positive or negative, can be drawn from this table.

Page: 51

Comment type: Technical

From the Report:

"While it has long been hypothesized that a rising global sea surface temperature would cause an increase in hurricane intensity, identification of any significant trend in the hurricane data is hampered by a short data record and substantial natural variability."

Comment:

There are important trends in the data that *have* been identified in the peer-reviewed scientific literature. First, using a homogenized satellite record, Kossin et al. (2020) showed statistically significant positive trends in the proportion of tropical cyclones that achieve major intensity. Second, the latitude where the lifetime maximum intensity (LMI) occurs has been increasing (Kossin et al. 2014; Studholme et al. 2022), although most of the signal is due to changes in the West Pacific (Kossin et al. 2016; Zhan and Wang 2017). Third, Bhatia et al. (2019, 2022) documented an increase in the frequency of "rapid intensification" events, which is when a TC increases in wind speed intensity by 30 knots or more in 24 hours. Klotzbach et al. (2022) and Garner (2023) also find an increase in the frequency of extreme intensification events within the observational record. These results are supported by modeling studies showing that intensification rates increase rapidly with temperature (Ramsay et al 2020). These findings are critical to TC impacts in U.S. coastal communities, given that such events are associated with larger forecast errors and larger negative impacts at landfall (Emanuel and Zhang 2016; Emanuel 2017b). Finally, Guzman and Jiang (2021) and Tu et al. (2021), both using global satellite observations of rainfall, found statistically significant positive trends in rainfall associated with TCs.

Page: 51

Comment type: Technical

From the Report:

"Process-based understanding also suggests that storm surges and rainfall from hurricanes should be increasing with warmer temperatures. However, the relatively small number of hurricanes with varying landfall locations and the complex dynamics associated with each storm preclude meaningful detection of change."

Comment:

In regards to rainfall, recent studies have identified observed increases in inland hurricane rainfall over land (Uehling and Schreck 2024, Maxwell et al 2021) and as noted above, the scientific community has also identified trends in TC rainfall rates over

the ocean (Guzman and Jiang 2021; Tu et al. 2021). This observed outcome is consistent with simple physics (more water vapor in saturated air at warmer temperatures, the Clausius-Clapeyron relation) and has been confirmed by many weather and climate models (Reed et al 2022; Stansfield and Reed 2021; Stansfield and Reed 2023; Stansfield and Rasmussen 2025). Moreover, rainfall-induced inland flooding impacts are rapidly increasing, as the top cause of hurricane fatalities has shifted sharply from storm surge to inland flooding since 2013 (Brennan et al. 2023). As a result, hurricanes increasingly pose a major inland flood risk regardless of their intensity at landfall. Empirical models show that economic damage increases with rainfall amounts and these damages are expected to continue to increase with warming (Gori et al. 2025).

For storm surge, recent research has shown in the observational record a likely increase in extreme surge levels in multiple regions along the U.S. coastline (Morim et al. 2025) as well as in Japan (Islam et al 2021). Additionally, numerous studies have shown how rising sea levels will directly translate to higher storm surges and also greater compound flooding from surge and rainfall (Garner et al., 2017; Muis et al. 2020, Gori and Lin, 2022; Mayo and Lin, 2022).

To ensure that Section 6.2 of the DOE CWG reflects an accurate knowledge of the impacts of climate change upon TCs, the following changes are recommended:

- Any framework that selectively identifies trends (or lack thereof) within the observational record of TCs, while overlooking other statistically robust trends in the record, should be abandoned.
- A more thorough review of the literature should be completed, and findings from modeling and theoretical studies assessing the impacts of climate change upon TCs should be incorporated into the document.
- The text should be revised to show that the proportion of TCs that reach major hurricane status has already increased, and is expected to continue to do so as the planet warms.
- The text should address the evidence for increasing TC intensification rates
- The chapter should discuss the scientific evidence showing that rainfall and associated inland impacts of TCs have already begun to worsen, and will continue to become more extreme with additional warming.
- The report should acknowledge observed and projected changes in sea level, and the fact that such changes in sea level will serve to amplify the severity of TC storm surge events.

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Comment on Section 6.3: Extreme weather: Temperature extremes

John M. Wallace, Chan-Pang Ng, and David Battisti

Section 6.3. Temperature extremes (*p. 52-60*)
The impacts of human-induced climate change are felt every day and at every location, but they are front-page news only when temperatures reach record-breaking levels. Seemingly paradoxically, the more extreme the high temperature event, the smaller the fractional contribution of climate change relative to that attributable to natural variability. The DOE CWG report's focus on extreme events, rather than considering the shift in the whole frequency distribution of temperature, is contrived to minimize the apparent influence of climate change. But the authors go a step farther: they misinterpret and distort the observations on extreme events to support their narrative.

Section 6.3.1 "Temperatures in the U.S. are becoming less extreme" (*p. 53-56*)
The argument that supports this outrageous claim is that over parts of the continental United States, the all-time high temperature records were set during the late 1920s and 1930s. Apart from a quote from NC4, which makes the same (factually correct) point, the report contains no mention of the Dust Bowl in the southern Great Plains, arguably the greatest environmental disaster in American history, brought on by agricultural practices designed to maximize short term profits without regard for the long-term consequences, e.g., see Egan and Lawlor (2006). The authors use the extremely hot, dry meteorological conditions observed during this slowly evolving tragedy as a benchmark for assessing whether human-induced global warming is detectable in statistics of extreme events, and they reject observational evidence based on analyses that exclude their benchmark era. In effect, they are treating the extreme weather conditions of the Dust Bowl era as if they were natural variability that should be expected to occur from time to time, even in the absence of human influences. In doing so, they are formulating their statistical analysis in terms of an inappropriate null hypothesis that cannot be falsified. This is rhetoric, not science.

Contrary to the authors' claims, the human-induced global warming signal is clearly discernible in all-time high and low temperature records over the continental United States and throughout the world, as shown in Fig. 1. Similar evidence was presented in Meehl et al. (2009) and the Summary for Policymakers of the WG1 Report of IPCC in 2021, but it is ignored in the DOE Report.

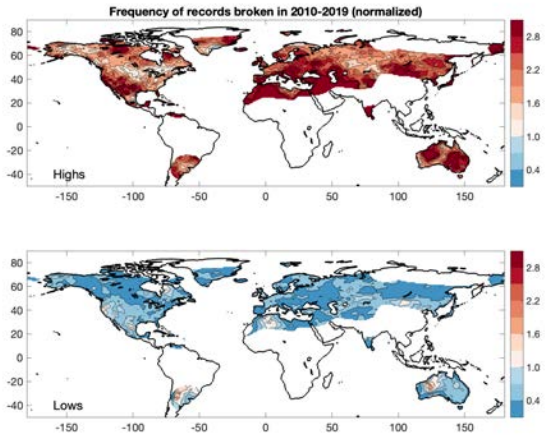

Figure 1. Numbers of all-time high and low temperature records that were set during the decade 2010-2019 divided by the expected values based on chance. Record high temperatures are being broken at a rate 2-3 times greater than expected by chance; record low temperatures are being broken at less than half of what would be expected by chance. Based on Berkeley Earth, 1920-2019.

When viewed from a global perspective, it becomes apparent that the spate of all-time high temperature records that were set during the late 1920s and 30s is a regional feature associated with the Dust Bowl. The high temperature records at the overwhelming majority of the grid points occurred recently, as shown in Fig. 2.

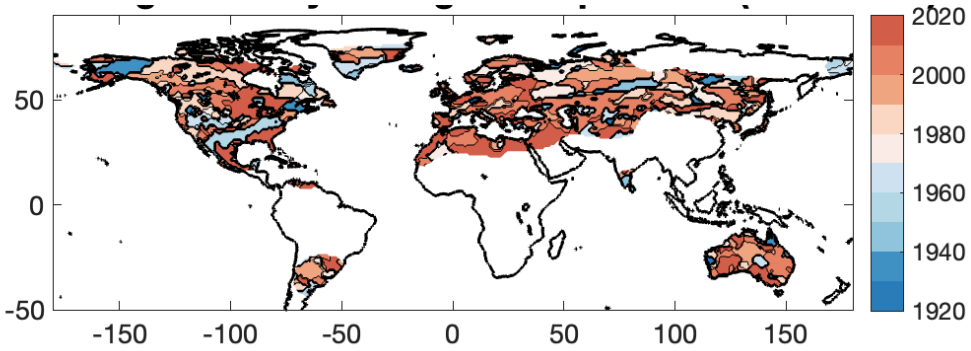

Figure 2. The decade in which the all-time high temperature record was set. Based on Berkeley Earth, 1920-2019.

6.3.2 Exceedances of a heat threshold (*p. 56-57*)

Based on the same bogus Dust Bowl benchmark criterion, the authors argue that the frequency of days with high temperatures above 95°F isn't changing. Unless the positive tail frequency distribution of temperature is contracting—and that's highly unlikely—the shifting of the frequency distribution of temperature toward higher values has to be accompanied by an increase in the frequency of days with temperatures above specified thresholds like 95°F. I can only conclude that here must be something wrong with analysis that was used to produce ***Figure.***

6.3.5 with the data that went into it. Our analysis, shown in Fig. 3, is indicative of a robust rising trend the parallels the rising mean temperature.[1]

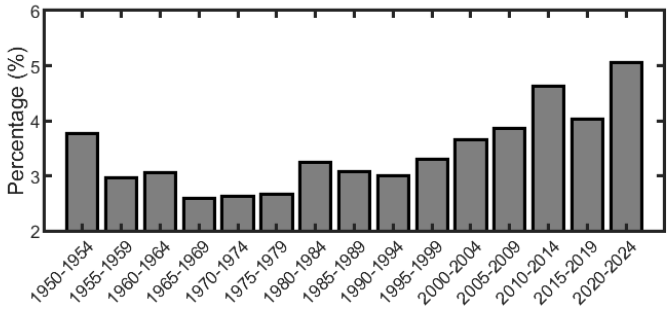

Figure 3 Fraction of 2.5° x 2.5° lat / lon gridpoint-days in the ERA5 reanalysis in which the daily maximum temperature exceeded 95°F.

6.3.3 Heatwaves (*p. 57-60*)

The same bogus Dust Bowl benchmark criterion is invoked again in this subsection to argue that the frequency of heat waves is not increasing. There's no denying that heat waves during the Dust Bowl era were beyond the pale of anything we've seen since that time, but that doesn't prove that human-induced global warming doesn't increase the frequency of occurrence of heat waves. Just as a rising tide lifts all boats, human-induced global warming lifts each day's temperature. If the local temperature has risen by, say, 3°F in response to global warming over the past 50 years, then a day on which the temperature would have reached 95°F fifty years ago would reach 98°F today, and the same is true for any other prescribed threshold. To deny that rising mean temperatures affect the statistics of heat waves is equivalent to denying the reality of human-induced global warming.

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[1] The four-year interval 1952-55 was indeed anomalously hot, especially in the central US, but not as hot as it was during the Dust Bowl. era.

Section 6.3: Extreme temperatures

Andrew E. Dessler[1], Karen McKinnon[2], Enrique Sànchez[3], Lucas R. Vargas Zeppetello[4]

Section 6.3: pages 52-60, Figures 6.3.3, 6.3.5, and 6.3.6
Comment Type: Technical
Comment: The Chapter 6 summary of the DOE CWG report states: "Most types of extreme weather exhibit no statistically significant long-term trends over the available historical record. While there has been an increase in hot days in the U.S. since the 1950s, a point emphasized by AR6, numbers are still low relative to the 1920s and 1930s." The executive summary (p. ix) states that "Most extreme weather events in the U.S. do not show long-term trends."

These are misleading claims that ignore the well-established connection between anthropogenic global warming and extreme temperatures. The intellectual framework employed by the DOE CWG ignores a wealth of peer-reviewed science linking anthropogenic global warming to changes in extremely high temperatures while focusing on trends, which conflates a decadal climate anomaly over the Eastern U.S. during the 1930s with the long-term global trend over the entire record. We address this issue in our "Intellectual Framework" section.

In addition, the DOE CWG report's use of non-homogenized station data is also inappropriate for understanding continental averaged trends in extreme temperatures. We address this issue in our "Data and Analysis" section and provide a complementary analysis that shows larger trends in extreme temperatures. **Based on the preponderance of peer-reviewed literature and our own analysis of gridded daily temperature data, there is little doubt that anthropogenic climate change driven by greenhouse gas emissions is making extreme heat more frequent and severe, both in the United States and across the globe.**

To correct these issues, the following changes must be made in the DOE CWG document:

1. Replace the problematic framework used here ("there is no trend in extreme heat in the U.S." implying that climate change is not making extreme heat more severe) with one that reflects the multiple processes that have contributed to climate variability in the U.S.
2. Accurately summarize the peer-reviewed literature that has demonstrated the fundamental physical link between long-term global warming and changes in extreme temperatures.

[1] Texas A&M University, [2] U.C. Los Angeles, [3] Universidad de Castilla – La Mancha, [4] U.C. Berkeley

3. Validate analysis using homogenized and bias-corrected data sets.
4. Acknowledge that a focus on the U.S. is not representative of the planet as a whole.

(1) Intellectual Framework

(a) Conflation of Timescales

The DOE CWG report repeatedly refers to the 1930s as a period of extremely high temperatures throughout the continental United States. From this, they make the statistically dubious leap that, because the 1930's extreme temperatures are comparable to today's temperatures (Fig. 1), then climate change cannot be contributing to more extreme heat today.

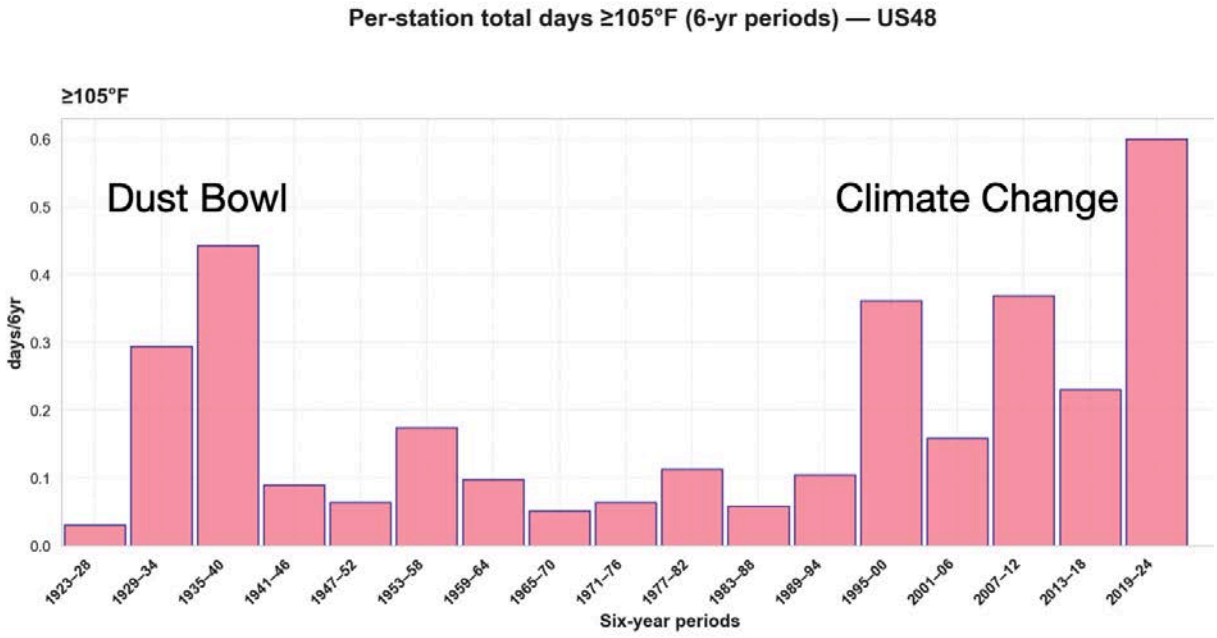

Figure 1: Number of days in 6-year periods (per grid point) that exceed 105°F. Details of this plot are provided in Section 2 of this comment.

However, this conclusion falls apart when crucial physical context is added. The critical missing piece in this analysis is recognizing that the extreme heat of the 1930s was driven by the unique and devastating conditions of the Dust Bowl era (Schubert et al. 2004, Cook et al. 2009). Massive agricultural mismanagement, unforced climate variability, and severe drought, caused the destruction of prairie ecosystems and created an environmental catastrophe that amplified heat extremes through altered land-surface interactions and reduced evapotranspiration (Donat et al. 2016a, pp. 190-191).

These specific conditions — widespread soil erosion, denuded landscapes, and the particular SST patterns of that decade — do not exist today. Today, the increases in extreme heat events are

occurring due to fundamentally different physics and can be confidently attributed to the enhanced greenhouse effect from human emissions (Seneviratne et al., 2021).

This case perfectly illustrates why relying on naive statistical analysis without considering underlying physical mechanisms leads to profound errors. A proper scientific assessment must account for the different drivers of extreme events across different time periods. In adding this context, the chart actually provides compelling evidence for climate change impacts rather than refuting them.

The claim that "For CONUS as a whole, the evidence in this section suggests GHG emissions have had little-to-no effect on heatwaves" is therefore misleading because it combines climate change with other physical drivers of climate variability, therefore ignoring key physical context.

A more correct interpretation of the results from the DOE CWG working group is that multiple processes have influenced temperature extremes in the continental U.S. over the last century, with anthropogenic climate change playing a dominant role in recent decades.

(b) Peer-Reviewed Literature

Analyses of extreme temperatures suggest that changes in extreme summertime temperatures follow changes in the median (McKinnon et al. 2016, 2024). **As noted by authors of the DOE CWG report, there has been an increase in the frequency, intensity, and duration of heat waves globally since the 1950s. In virtually all of the peer-reviewed literature, long-term anthropogenic climate change driven by greenhouse gas emissions has been invoked to explain this increase** (Perkins-Kirkpatrick & Lewis, 2020, Seneviratne et al. 2021). One exception to this pattern is the U.S. Midwest where extremely high temperatures have decreased in intensity over the recent observational record; however this has been shown to be a response to agricultural intensification that is independent from the influence of anthropogenic global climate change (Mueller et al. 2016, 2017).

(2) Data and Analysis

The DOE CWG report's analysis of temperature extremes relies exclusively on daily data from the U.S. Historical Climate Network (USHCN). Multiple studies have shown that time of observation bias, widespread station moves, equipment changes such as the transition from liquid-in-glass thermometers to MMTS sensors, and changes in site surroundings have introduced both abrupt and gradual shifts in measured temperatures unrelated to long-term anthropogenic climate change (Menne et al. 2009, Lawrimore et al. 2011). The DOE CWG report claims that "Although there are certainly errors in the dataset, including unresolved spurious warming due to [urban heat island] effects that especially bias Tmin records, this data

set is sufficiently accurate for assessing trends in Tmax heat extremes." (p. 53). There is no citation for this claim. However, the sentence only acknowledges problems associated with the urban heat island phenomenon, ignoring problems from the many other biases in the data set, such as changes in equipment, changes in time of observation, changes in station location. While station datasets such as USHCN and GHCN are used for climate studies given their uniquely long records, care must be taken to assess and quantify the potential influences of inhomogeneities, and results should be compared across multiple datasets when possible.

(a) Trends in Extreme Events Using Daily, Homogenized Data

The DOE CWG report concluded that there were more hot record-setting days in the 1930s than any other decade in the observational record (reproduced as Fig. 2a below), and this forms key evidence for their claim that "Most extreme weather events in the U.S. do not show long-term trends." However, the dataset they used in the DOE report (USHCN) includes many biases that have not been removed, as the DOE CWG report acknowledges.

We have conducted an identical analysis to the one performed in the DOE CWG report using daily gridded data from Berkeley Earth (Fig. 2b). The Berkeley Earth dataset developed by Rhode et al. (2013) has been extensively used in the peer-reviewed literature (e.g., Perkins-Kirkpatrick & Lewis, 2020, Patel et al. 2024). Importantly, the Berkeley dataset adjusts for the well-known biases in the USHCN data.

The Berkeley figure (Fig. 2b) shares some key features with the DOE CWG report's analysis (Fig. 2a), particularly the high number of temperature records in the 1930s.

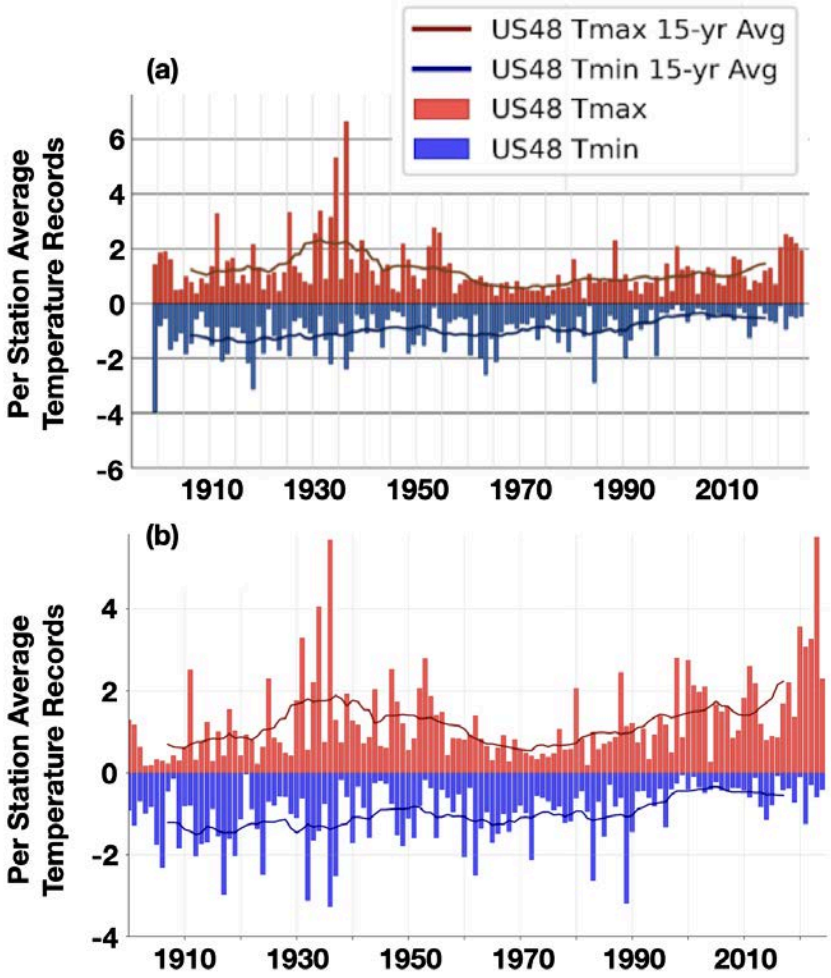

Figure 2. Number of daily record high (red) and low (blue) temperatures for warm and cold seasons in the Continental United States (US48). Solid lines represent the 15-year running, centered average. Panel (a) is adapted from Figure 6.3.3 of the DOE CWG report and is based exclusively on USHCN stations. Panel (b) is the same analysis conducted on the homogenized and gridded Berkeley Earth data. In this plot, the grid points are considered "stations".

However, one key difference stands out: **in our calculation, the most recent few years have had as many record-breaking high temperatures as the 1930s. In fact, the year with the most record-breaking hot days is 2023.** This is also true in the 15-year running average, where our analysis shows that the most recent 15-year period is the first where more than two temperature records were set per station per year.

We conclude that an important conclusion of the DOE CWG report, "While there has been an increase in hot days in the U.S. since the 1950s, a point emphasized by AR6, numbers are still low relative to the 1920s and 1930s" (p. 46) **is very likely not correct.**

The consequences of the DOE CWG report's reliance on non-homogenized station data also influence the analysis presented in Fig. 6.3.5 and 6.3.6, that are claimed to illustrate that the 1930's was the decade with the most exceedances of 95˚F and the most heat wave days. In our analysis of the Berkeley Earth data, we find that the exceptional warmth of the 1930s is not as extreme as in the most recent decade (Figs. 3 and 4).

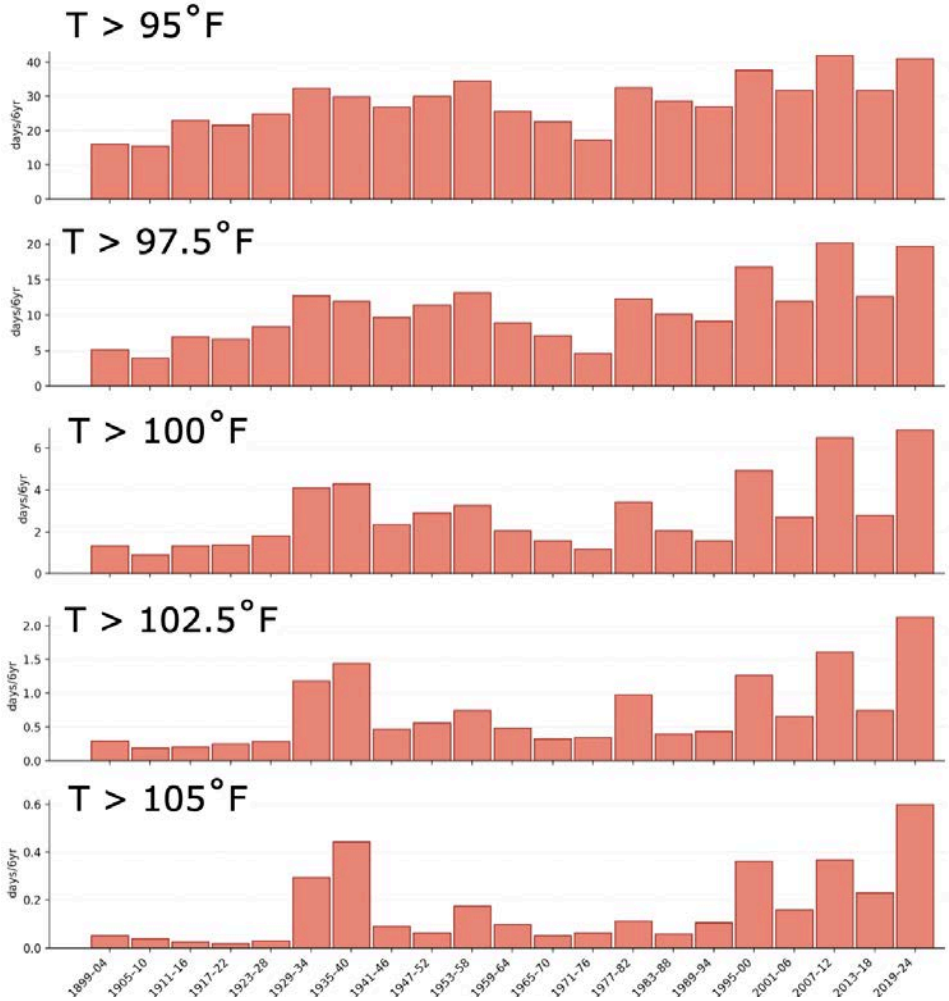

Figure 3: Number of days per grid point per six-year period where temperature exceeded a particular threshold from Berkeley Earth daily data averaged over the Continental U.S.

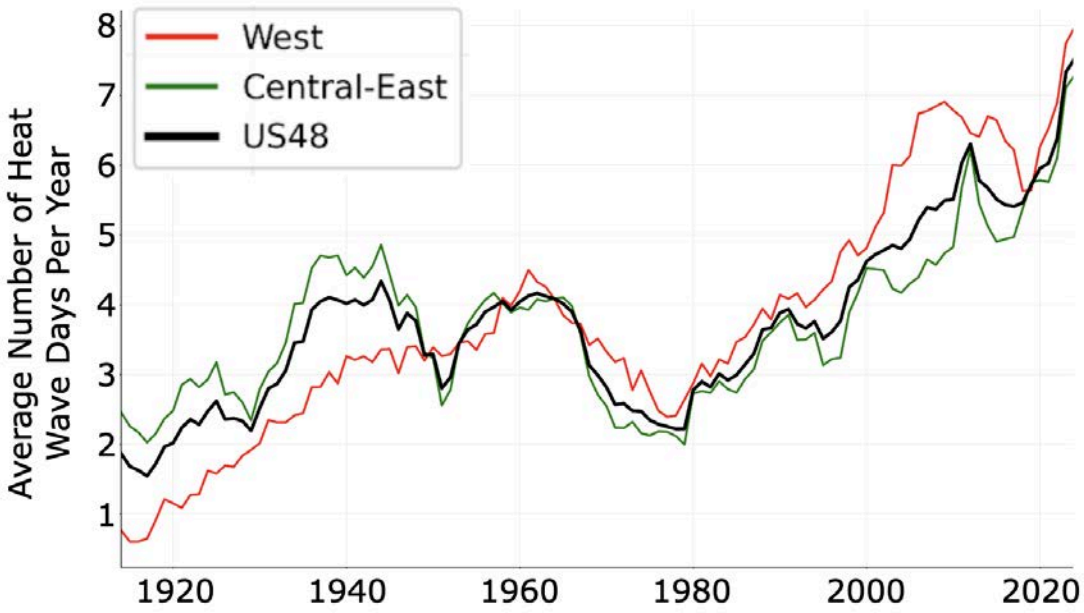

Figure 4: Per-grid-point average number of heat wave days per year from daily Berkeley Earth observations. The black line shows the average across the Continental U.S., the red line shows the average west of the Rocky Mountains, and the Green line shows the average east of the Rocky Mountains.

In general, our analysis of the Berkeley Earth data demonstrates that extreme high temperatures have become more frequent over the Continental U.S. over the past two decades than at any point in the previous observational record. **Thus, the conclusion of the DOE CWG report that "Most extreme weather events in the U.S. do not show long-term trends" aside from being misleading and ignoring the physical processes responsible for natural climate variability (see Section 1), is not a robust conclusion because it depends on the data set and the metric under consideration.**

(b) An Absent Global Perspective

The focus of Section 6.3 of the DOE CWG report is on temperatures in the Continental U.S. This is a clear example of **cherry picking**, defined to be an intentional selection of a small subset of the data that gives a different conclusion to the entire data set. To illustrate this, we show global summertime average high-temperatures from the Berkeley Earth dataset from 1930-1939 and from 2014-2024 (Figs. 5a and b, respectively).

Globally, summertime high temperatures are consistently higher in the last decade than they were in the 1930s (Fig. 5c). The only exception is the Eastern United States, where temperatures were warmer during the 1930s due to known decadal climate fluctuations discussed earlier (Schubert et al. 2004, Cook et al. 2009, Donat 2016a, 2016b) and where temperatures have been cooler

than normal over the past few decades due to agricultural intensification (Mueller et al. 2016, 2017).

The decadal climate anomaly that drove an increase in extremely high temperature during the 1930s was tightly confined to the Eastern United States and does not represent a global pattern of how long-term anthropogenic climate change drives increases in extremely high temperatures.

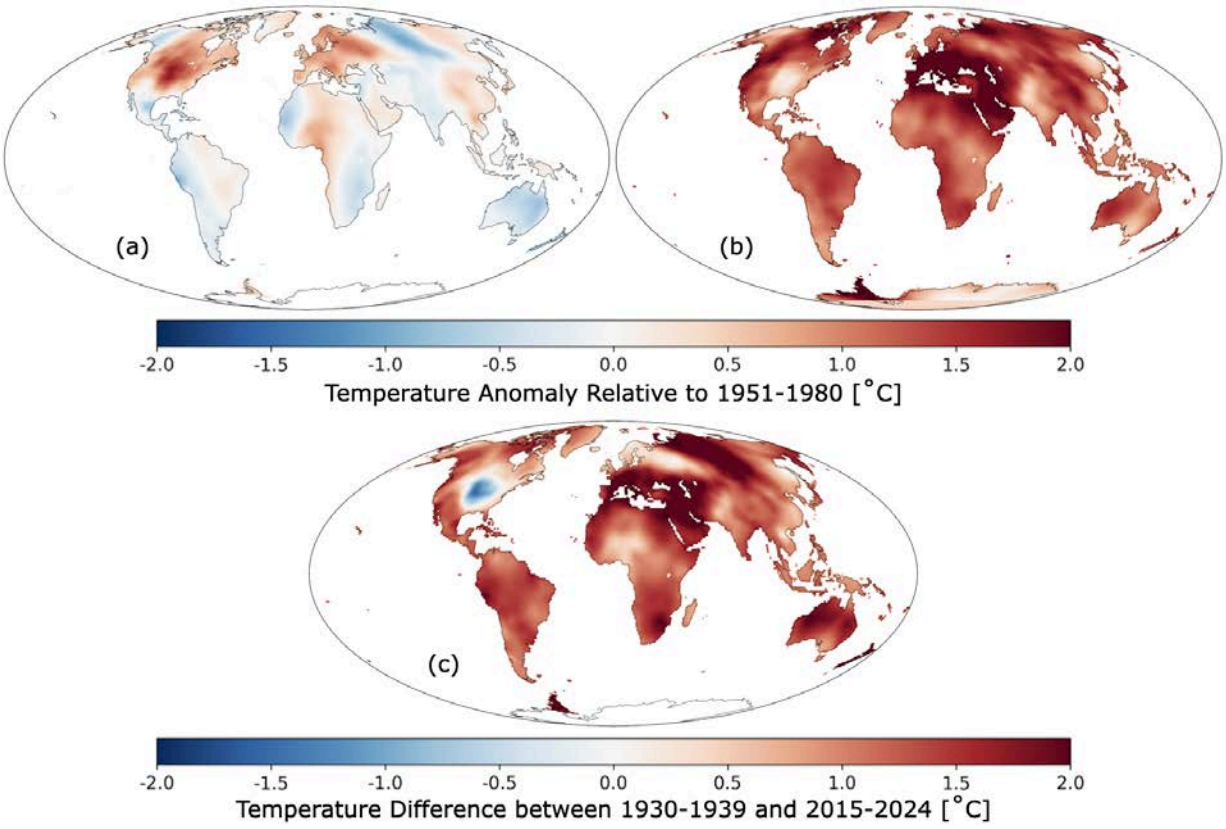

Figure 5: Panels (a-b) show summertime (June, July, August) average high temperature anomalies relative to the 1951-1980 reference climatology for 1930-1939 and 2015-2024 periods, respectively. Panel c shows the difference between panels a and b.

To demonstrate the regional nature of the 1930s heat anomaly in the Eastern U.S., we have reproduced Figs. 6.3.3, 6.3.5, and 6.3.6 (Figs. 6-8), but included the entire land surface of the northern hemisphere midlatitudes (rather than only considering the Continental United States as is done in the DOE CWG report). Investigating at a larger scale than the U.S. dilutes the impact of the Dust Bowl in the 1930s, and makes the impact of climate change much more apparent.

Our results align with the peer-reviewed literature on heat waves (see above) and demonstrate that severe heat is getting worse throughout the northern hemisphere midlatitudes. We note again that warm temperatures in the 1930s due to a particular set of circumstances is fully compatible with the fact that climate change is making today's extreme heat more intense (see Fig. 1).

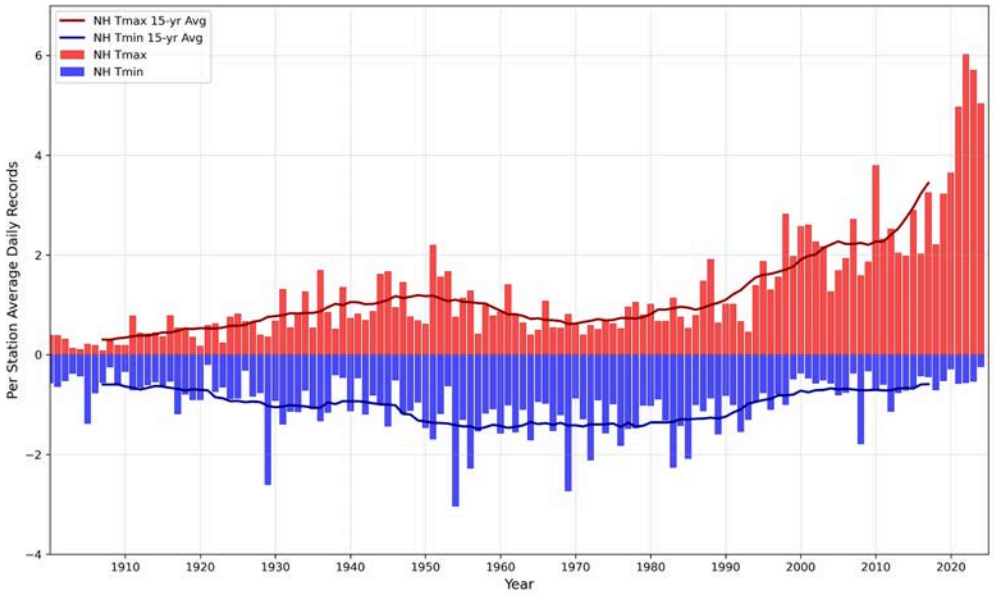

Figure 6: Record-setting land temperatures for the entire northern hemisphere mid-latitudes (24°N-50°N) calculated with the same approach as Fig. 2 using the Berkeley Earth Data.

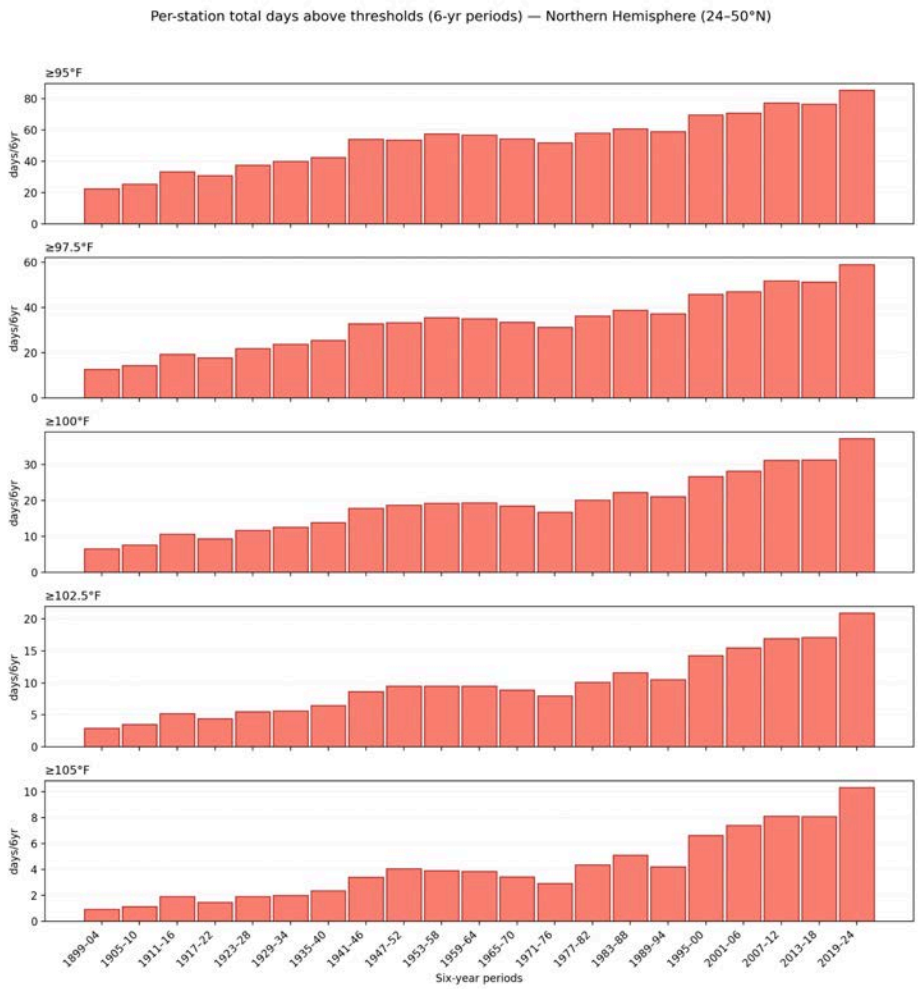

Figure 7: Exceedances of particular temperature thresholds averaged across stations in the Northern Hemisphere land midlatitudes (24°N-50°N). Analysis was identical to that performed in Fig. 3, except that we used all Berkeley Earth grid cells in the Northern Hemisphere midlatitudes.

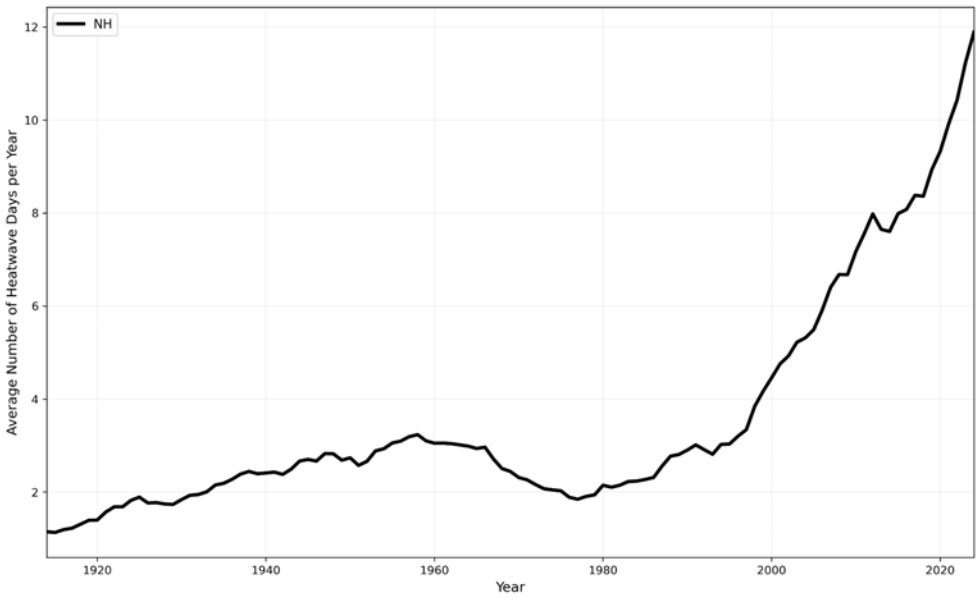

Figure 9: 15-year running average of number of heat wave days per station per year averaged across all Berkeley Earth grid cells in the Northern Hemisphere land midlatitudes (24°N-50°N). Analysis is identical to that presented in Fig. 4, except for the number of grid cells included in the Analysis.

Reproducibility

Many of the details of the analysis conducted by the DOE CWG report authors are not provided in section 6.3. The DOE CWG report uses an enhanced USHCN dataset created by merging data from nearby stations when original stations had gaps or closures and regridding to a 0.5˚ grid over the Continental U.S. However, there is no documentation on the details of this important bias correction procedure. In addition, they did not archive the code that was used to produce the figures critiqued in this section. **This lack of documentation of the data and methods used in the DOE CWG report means that the work is fundamentally unreproducible. As a result, the analysis likely does not adhere to the "gold standard science" Executive Order or the Information Quality Act.**

Conclusions

The report's central claim that "Most types of extreme weather exhibit no statistically significant long-term trends over the available historical record" is **fundamentally misleading** when our physical understanding of the 1930s is considered, and **appears to be inaccurate** for some metrics or data sets, based on our analysis of homogenized observations.

The peer-reviewed science is clear: **long-term anthropogenic climate change is making extreme heat more extreme in both the Continental United States and globally.**

Physical Understanding: The DOE CWG report's result that no significant trends in U.S. extreme temperatures exist across the instrumental record rests entirely on the decadal 1930s heat anomaly in the Eastern U.S. However, this anomaly is well-understood as resulting from specific regional factors including drought, land mismanagement, and natural variability during the Dust Bowl. The fact that the 1930s were warm in the Eastern U.S. does not contradict the scientific consensus that long-term anthropogenic climate change is exacerbating extremely high temperatures today.

The implication of the DOE CWG report that "the 1930s were warmer than today, so climate change is increasing extreme temperatures" is supported only by ignoring the physical understanding of different timescales of climate variability. **Any future version of this report must incorporate fundamental knowledge of multiple timescales related to climate variability. They should also acknowledge that simple physical arguments assure us that climate change is indeed making extreme temperatures more extreme.**

Data Quality Issues: The DOE CWG report relies on daily USHCN station data that contain well-documented non-climatic biases unsuitable for climate analysis, including time of observation bias, station relocations, equipment changes, and urban heat island effects. While the authors of the DOE CWG report did re-grid the data to ameliorate sampling errors, they provided no documentation of this procedure and did not execute a homogenization procedure to remove other biases. In personal communications between one of us (AED) and the authors of the DOE CWG report, the DOE authors claimed that the biases are actually small but provided no evidence to support that other than anecdote, motivated reasoning, and arm waving. **They must improve this analysis with better data and quantitative and comprehensive characterization of the errors.**

Corrected Analysis: Using the spatially uniform, homogenized Berkeley Earth dataset reveals that recent years have experienced more extreme heat days than the 1930s peak. This finding directly contradicts the DOE CWG report's conclusions across multiple metrics including record-breaking temperatures, days above 95°F, and number of heatwave days.

Global Context: Expanding the analysis beyond the continental U.S. to northern hemisphere mid-latitudes demonstrates a clear warming trend, with the last decade consistently showing higher temperatures than the 1930s across most regions. This broader perspective reveals that focusing solely on the U.S. obscures the global warming signal. **They should acknowledge that their analysis is built on cherry picking and report results for larger regions, as we did in this comment.**

References

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Perkins-Kirkpatrick, S. E., and S. C. Lewis. "Increasing trends in regional heatwaves." *Nature communications* 11.1 (2020): 3357.

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Seneviratne, S.I., et al., 2021: Weather and Climate Extreme Events in a Changing Climate. In *Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 1513–1766.

Data Access:

Code to reproduce the figures can be found here. The Berkeley Earth data are found here. USCRN data used in the DOE CWG report are found here.

Response to Chapter 6, Section 6.4: Extreme Precipitation

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Dr. Ben Clarke
Centre for Environmental Policy, Imperial College, London, UK

Dr. Christopher M. Colose

This section of the DOE CWG report focuses on the author's analysis of extreme precipitation trends across a subset of stations in a few regions of the continental U.S. We note that extreme precipitation is of interest in the physical climate system since increases in the heaviest downpours are expected to occur in a warming climate, a longstanding prediction that has been borne out observationally (e.g., Westra et al., 2013, 2014; Fischer and Knutti, 2016; Dong et al. 2020; Paik et al., 2020; Fowler et al., 2021; Sun et al., 2021). This is because more water vapor can be present in a warmer atmosphere (at approximately the Clausius-Clapeyron scaling of 7% K^{-1}), far greater than changes in *average* precipitation. This upper bound on water holding comes into play in extreme events where water is converging into a region and a "parcel" of air empties all of its moisture. Nonetheless, this statement is sensitive to the time horizon considered (e.g., hourly vs 1-day vs 5-day) and it is not true at the level of every weather station (especially since extreme events are rare by definition) and a larger spatial scale is required to isolate the signal-to-noise ratio for detecting climate change effects. In this section, the authors only consider 3-day and 5-day rainfall records, which does not cover the complete picture of risk in extreme events, for example with flash flooding. Below, we highlight these and other specific issues of the DOE CWG section 6.4.

Page: 60, Box: Perils of short data record
Comment Type: Reference
Comment:
Porter et al (2011) is a 200-page document on hypothetical megastorm scenarios designed to improve hazard preparedness. The authors of the DOE report are referring to one sentence (p. 2) which states that there is geological evidence of six megastorms that caused large floods prior to European settlement. Porter et al. assert that the

1861-62 storms were “probably not the worst” and then refer to the sedimentary layers indicating these six past megastorms. Porter et al. do not provide any quantitative indication of the intensity of these storms. Therefore, the claim that “Porter et al. (2011) discovered that in the past 1,800 years at least six megastorms were more intense than the devastating 1861-62 ARkStorm” is not supported by Porter et al. (2011).

Page: 61
Comment Type: Reference
Comment:
The literature regarding rainfall trends are all self-citations (except for the NCAs which are used to introduce the topic). The report authors do not reference any of the published peer-reviewed literature on the topic, of which there is plenty.

Other studies cited by the National Climate Assessment and IPCC using all the available information find increasing trends across most of the CONUS in most seasons (e.g. Kunkel et al., 2020; Kirchmeier-Young & Zhang, 2020; Dunn et al., 2020; Sun et al., 2021)

Page: 61
Comment Type: Technical
Comment:

“Interestingly, the regional variations indicate that the largest increases in extreme precipitation events are in the Northeast and the smallest in the West, a pattern counter to the changes in temperature extremes”
It is unclear what point the authors are trying to make here. Precipitation extremes and temperature extremes are caused by different physical mechanisms, so it is not expected that local extreme precipitation trends will follow local extreme temperature trends.

Page: 62, 63, Figure 6.4.1- Figure 6.4.3
Comment Type: Technical
Comment:

In their analysis, the report authors average over 29 stations on the Pacific coast to conclude there is no tendency for extreme precipitation events to become more frequent over time. They conclude the same from 24 southeast and 27 northeast stations, at least when averaging over all stations in the analysis. Of the more than 2000 weather stations in the US, they use approximately 80. Although the authors did not cherry-pick these stations, it still undersamples spatially. Jorgensen & Nielsen-Gammon (2024) noted this in reference to the southeast domains, referring to McKitrick & Christy, (2019): "The majority of these stations happen to be located in those parts of Mississippi, Alabama, Georgia, and northern Florida where the historic trends are relatively weak and nonexistent, and none of them are in the parts of Texas, Louisiana,

and North Carolina with the largest trends." Sun et al.(2021) also show that over North America, there is clearly a larger number of station-level increases than decreases in maximum amount of precipitation accumulated over 1 day (Rx1day) and 5-day (Rx5day) precipitation, although it is true that the western part of the country exhibits a different pattern. We also note that extreme precipitation evaluation is sensitive to the metric of extremeness. For example, Jorgensen & Nielsen-Gammon showed that daily rainfall intense enough that it only has a 1% chance of happening in any given year has markedly increased in the southeastern US. For the Pacific region, given that we expect latitudinal changes in Atmospheric River landfalling (e.g. Payne & Magnusdottir, 2015; Shields & Kiehl, 2016), averaging over all stations may lead to weak trends because some locations may experience increasing trends while others experience decreasing trends.

Page: 62
Comment Type: Reference
Comment:
See previous Porter comment.
The reference to the six previous megastorms and the 19th century ARkStorm are examples of cherry-picking. While the 1861-1862 event was significant, there have been plenty of impactful Atmospheric River events since which are ignored by the report. For example:

- December, 2010, parts of California received 250-670mm (10-26 in.) (Ralph & Dettinger, 2012).
- May, 2010, Nashville received 344.7mm of rain in 2 days and 11 people died (Moore et al, 2012).
- The 2017 Oroville Dam Spillway Incident (Henn et al, 2020).
- January 2021, Big Sur landslide (Zou et al, 2023).

Page: 62
Comment Type: Technical / response
Comment:
The authors select the 26 wettest 5-day intervals, which they consider to represent a one-in-five-year event. This would be the case if they were looking at only the wettest 5-day period in each year (or, as is often more appropriate, each season), in which case the events can be assumed to be essentially independent. However, it's not clear if consecutive very wet days – or wet days at multiple neighbouring stations simultaneously – have been declustered or simply treated as if they were independent observations. Without such declustering, very large storms will appear in the sample multiple times, introducing biases towards years with such events.

The report authors claim that 'if there are no trends in precipitation, then the total number of these events across all stations should be evenly spread over the years'. However, this part of the analysis has not looked at trends in precipitation volumes, only

counted exceedances (possibly including clusters of exceedances) of a high threshold. Long-duration events like these are expected to be strongly affected by changes to the dynamic component of precipitation i.e. where are the weather systems and do they stall (Pfahl et al, 2017), which is dominated by natural variability. Furthermore, when events are pooled indiscriminately in this way without considering whether they are likely to arise from the same dynamical physical processes (e.g. hurricanes, severe thunderstorms, fronts, atmospheric rivers), potentially meaningful trends in subsets of the data can be masked. The lack of an identifiable trend in this analysis is therefore not surprising.

This is an unusual approach with no cited basis in the literature other than self-citation, and no written justification. There is also only one metric used, which means it is impossible to tell how the results are sensitive to the choice of metric used.

Page: 64
Comment Type: Technical
Comment:
"We use 3-day totals here as this produced the largest temporal variations in time."
There are two possible interpretations of this remark, and it's not clear which is intended by the authors. The 'largest temporal variations in time' may mean the strongest trend (i.e. the largest variation in the mean trend over time); however, it may also mean that the variability (noise) in the time series was larger. For either interpretation, it's concerning that the authors change their analysis method without properly discussing the sensitivity of the results to the length of accumulation studied.

Page: 65, Figure 6.4.5
Comment Type: Technical
Comment:
It's not clear why the authors only present changes in average rainfall per event for one region. The caveats discussed above concerning the possible double-counting of large events, and treating events that occur in different seasons and regions, without accounting for prevailing modes of climate variability, as if they are part of a homogeneous population, apply here also. The authors refer to Figure 6.4.5 (average precipitation per event) in support of their finding that there was no regional pattern in the number of events, although Figure 6.4.5 says nothing about the frequency of events.

Page: 63-65
Comment Type: Technical
Comment:
There are several key weaknesses of this method beyond those already highlighted:

- The analysis studies change over time, rather than changes with greenhouse gases or global warming, as established attribution methods do. That means that the natural variability expected in the 19th and early 20th centuries, when human

influence was relatively small, could hide the more recent strong trends as the world has warmed.

- The analysis does not account for the effects of internal climate variability, which can mask trends. This was used as a criticism of extreme event attribution by the authors in section 8.6 (p. 95).
- Many of the conclusions in the section are drawn from a visual inspection of the time series presented. This is not a scientifically robust approach.
- The authors rely solely upon statistical significance to determine whether a trend, and (by their implication) climate change influence, is present in the data. The American Statistical Association Statement on p-Values (i.e. statistical significance) warns that: 'Scientific conclusions and business or policy decisions should not be based on only whether a p-value passes a specific threshold' (Wasserstein & Lazar, 2016). Statistical significance alone cannot prove nor refute the cause of a trend or lack thereof.

Page: 66
Comment Type: Reference
Comment:
"Jong et al. examined CMIP6…fewer such systems in the 21st century…This conjecture is not seen in Fig 6.4.5."
Figure 6.4.5 presents data from 1890-2024, which is predominantly the 20th century. It therefore cannot be used to refute projections of changes that are expected to occur in the 21st century, in a further warming climate. Furthermore, it's likely that the data represented in Figure 6.4.5 combines precipitation from hurricane and non-hurricane events with rainfall driven by other processes, which may exhibit different trends. A more careful analysis would be required to understand how rainfall associated with particular circulation patterns and weather types is changing. Furthermore, they claim that the amount per event has remained steady over the 135-year period but previously referred to a weakly increasing trend. Furthermore, the Jong et al. reference cited by the DOE CWG document, in its very first sentence (and echoed throughout the paper), states: "The Northeast United States (NEUS) has faced the most rapidly increasing occurrences of extreme precipitation within the US in the past few decades." Many other papers also demonstrate this (e.g., DeGaetano et al., 2020).

Page: 66, summary paragraph
Comment Type: Technical
Comment:
The authors conclude that the claim that extreme short-duration rainfall events are becoming more frequent or intense is not supported by their results. However, their analysis examined 5-day and 3-day rainfall totals over a fraction of the US. They do not present results on short-duration rainfall (usually considered to be less than 1-day) and therefore cannot draw conclusions about short-duration rainfall from their analysis. Finally, as discussed above in the comment on Figure 6.4.1, characterising the small

number of stations analysed here as providing ‘nationwide’ coverage is extremely misleading, and to extrapolate from such a limited analysis to statements about trends over the whole of the United States is scientifically indefensible.

To address these comments, the following changes must be made:

- Summarize peer-reviewed literature beyond the authors’ own work on extreme rainfall trends.
- Expand the number of stations analyzed to incorporate a broader spatial domain.
- Expand the extreme rainfall metrics used to include short-duration rainfall extremes and to explore the sensitivity of the results to the choice of metric.
- Revise the trend attribution method to account for natural variability and test whether extreme rainfall changes with a direct indicator of greenhouse gases or global warming such as temperature or CO2.

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Response to Section 6.5 Tornadoes

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Daniel R. Chavas, Purdue University

Summary

In Section 6.5, Tornadoes, the DOE CWG report misquotes Intergovernmental Panel on Climate Change (IPCC) AR6, and omits key findings on increased variability, eastward shifts, and winter clustering of tornadoes. Recent peer-reviewed studies confirm these changes and their implications for rising risk in more vulnerable regions. DOE should correct its AR6 citation, incorporate the full body of evidence, and revise its conclusions to reflect observed variability and shifting tornado distributions.

Comment 1.

Page: ix
Comment Type: Technical

Comment: The DOE CWG executive summary states increased frequency of tornadoes are "…not supported by U.S. historical data." This statement is missing nuance. As detailed in comments 2 and 3 below, observational data indicate tornadoes have clustered into larger outbreaks and have shifted eastward and increased over the Southeast U.S. in the winter.

Action: The DOE CWG authors should revise this sentence after a more rigorous assessment of the observational data and relevant scientific literature (see also action items in comments 2 and 3 below).

Comment 2.

Pages: 66-67, Figure 6.5.1
Comment Type: Technical

Comment: The authors of the DOE CWG report selectively quote chapter 11 of the IPCC Sixth Assessment Report (AR6; Seneviratne et al. 2021). The two sentences they quote are on page 1595, not 1594 as mentioned in the DOE CWG report. Moreover, the second sentence is only partially quoted. The full statement on observational trends in AR6 is below. The part in bold was left out of the DOE CWG report.

"…observational trends in tornadoes, hail, and lightning associated with severe convective storms are not robustly detected due to insufficient coverage of the long-term observations. There is medium confidence that the mean annual number of tornadoes in the USA has remained relatively constant, **but their variability of occurrence has increased since the**

1970s, particularly over the 2000s, with a decrease in the number of days per year, and an increase in the number of tornadoes on these days (high confidence)."

The part in bold is a significant omission. An increase in the variability was observed by Brooks et al. (2014), and this continues to be found in the literature (Tippett et al. 2016; Graber et al. 2024). Figure 1b from Graber et al. (2024) is reproduced below.

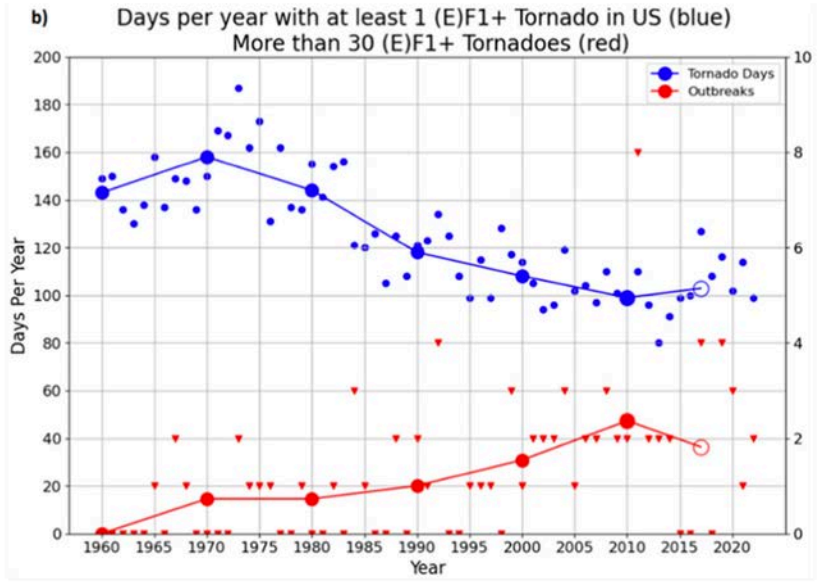

Figure 1b from Graber et al. (2024).

Action: The DOE CWG authors need to fully quote AR6 to include the observed increase in variability with tornadoes clustering into larger outbreaks.

Comment 3.

Pages: 66-67
Comment Type: Technical

Comment: The authors of the DOE CWG report omit how tornadoes have shifted eastward to become more common in the Midwest and especially the Southeast in winter. AR6 states on page 1595:

"There has been a shift in the distribution of tornadoes, with increases in the mid-south of the USA and decreases over the High Plains (Gensini and Brooks 2018)."

Recent analyses of both tornado data and tornado-favorable environments have confirmed this eastward and winter shift (Coleman et al. 2024; Graber et al. 2024; Strader et al. 2024). While a direct link from tornadoes to anthropogenic climate change has not been established, these regional trends are consistent with modeling studies showing severe thunderstorms in general shifting systematically eastward and earlier in the season in a warmer world (Ashley et al. 2023), suggesting that this link is plausible. Southeast tornadoes more frequently occur at night and

hence are much deadlier because people are less likely to take shelter (Strader et al. 2022). Moreover, the increase in tornado days over the Mid-South region coupled with societal exposure changes has increased the housing exposure threefold since 1980 (Strader et al. 2024).

Action: The DOE CWG authors need to include the observed shift in distributions of tornadoes and cite the relevant literature that has been published since AR6.

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Section 6.6: Flooding

Edited by A.E. Dessler, Texas A&M University

6.6.1 Summary..... 2
6.6.2 Addressing Each Statement in the DOE CWG Report..... 2
6.6.3 Synthesis of Literature Based on Best Available Science..... 4
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6.6.1 Summary

The assessment relied solely on the IPCC AR6, taking quotes out of context, and an outdated national climate assessment report (NCA4 instead of NCA5). It moreover failed to include any of the peer-reviewed studies on climate change and flooding.

The organization of this section is as follows: Section 6.6.2 reviews what the DOE CWG report did say followed by comments on what was not correct, left out, or otherwise lacking and what is needed to reconcile. Then section 6.6.3 summarizes best available science that the DOE report should have included in order for it to be a complete and unbiased summary of observations and projected climate change impacts on flooding. This revision should specify the different contributors to flooding, including the different ways that climate change can affect flooding (see section 6.6.3), such as highlighted by the 5th National Climate Assessment:

> “Precipitation changes do not have a one-to-one relation with flooding. Many factors influence floods, including short- and long-term antecedent moisture conditions, presence of frozen soils, snowpack accumulation, rain-on-snow events, storm tracks, and rainfall rates.” (NCA5, USGCRP 2023).

6.6.2 Addressing Each Statement in the DOE CWG Report

Comment 1.

Page Number in DOE CWG Report: 67
Statement in DOE CWG Report (bolded and italicized text is kept as in the report):

> “AR6: **The SREX assessed *low confidence* for observed changes in the magnitude or frequency of floods at the global scale**. This assessment was confirmed by the AR5 report. The SR15 found increases in flood frequency and extreme streamflow in some regions, but decreases in other regions. . . [H]ydrological literature on observed flood changes is heterogeneous, focusing at regional and sub-regional basin scales,

making it difficult to synthesise at the global and sometimes regional scales. (Chapter 11.5)"

Comment type: technical

Comment: As stated in the second part of the quote from AR6, it is difficult to synthesize flooding changes at the global scale due to the heterogeneity of scales being observed. Indeed, as with drought (see response to Section 6.7), flooding is heterogeneous, and therefore climate change impacts on flooding are also heterogeneous (NCA5). Within the U.S., multiple regions have experienced an increase in peak streamflow and flooding, including the eastern Dakotas (NCA5, 25-10), the Central U.S., and the Northeastern U.S. (Davenport et al., 2021; Mallakpour and Villarini, 2015).

Comment 2.

Page Number in DOE CWG Report: 67

Statement in DOE CWG Report (bolded and italicized text is kept as in the report):

"AR6: [T]he seasonality of floods has changed in cold regions where snowmelt dominates the flow regime in response to warming (*high confidence*). Confidence about peak flow trends over past decades on the global scale is ***low***. (Chapter 11.5)"

Comment type: reference

Comment: The citation from AR6 leaves out the second part of the statement (underlined), that points to regional differences in flooding related to runoff. Notably, the rest of the statement identifies the north-East US as a place where such flooding has *increased*:

"In summary, the seasonality of floods has changed in cold regions where snowmelt dominates the flow regime in response to warming (*high confidence*). There is *low confidence* about peak flow trends over past decades on the global scale, <u>but there are regions experiencing increases, including parts of Asia, Southern South America, north-east USA, north-western Europe, and the Amazon, and regions experiencing decreases, including parts of the Mediterranean, Australia, Africa, and south-western USA</u>."

Comment 3.

Page Number in DOE CWG Report: 68

Statement in DOE CWG Report (bolded text is kept as in the report):

"NCA4: Trends in extreme high values of streamflow are **mixed** across the United States. Analysis of 200 U.S. stream gauges indicates areas of both increasing and decreasing flooding magnitude **but does not provide robust evidence** that these trends are attributable to human influences (pp. 240-241)."

Comment type: technical, reference]

Comment: Flooding is highly linked to precipitation patterns and extreme events, which is observed (see e.g. Figures 7.1-7.4 in the USGCRP 2017 report and referenced literature therein (Easterling et al. 2017)) and projected (Figures 7.5-7.8 in the same report) to change across most of North America (also see our comments to section 6.4 of the CWG report). Another important factor influencing flooding is sea level rise, which is commented on in our response to Chapter 7 of the CWG report.

Due to the complexity of factors and regionality of what controls the severity and frequency of flooding, it is necessary to provide a more nuanced and comprehensive report of observed and projected changes. The lack of robust evidence that flooding trends are attributable to human influence stems from this complexity and heterogeneity across regions and scales, but it does not change the fact that the patterns are changing. From the USGCRP 2017 report, p. 241 (Wehner et al. 2017) - reference numbers in citation are from the USGCRP report:

> "Recent analysis of annual maximum streamflow shows statistically significant trends in the upper Mississippi River valley (increasing) and in the Northwest (decreasing).[44] In fact, across the midwestern United States, statistically significant increases in flooding are well documented.[78, 82, 83, 84, 85, 86, 87, 88] These increases in flood risk and severity are not attributed to 20th century changes in agricultural practices[87,89] but instead are attributed mostly to the observed increases in precipitation shown in Figures 7.1 through 7.4.[78, 84, 89, 90] Trends in maximum streamflow in the northeastern United States are less dramatic and less spatially coherent,[44, 80] although one study found mostly increasing trends[91] in that region, consistent with the increasing trends in observed extreme precipitation in the region (Ch. 6: Temperature Change).[34, 80]"

Further, pulling only from NCA4 when there is a more recent National Climate Assessment available (NCA5) is not appropriate. Indeed, many flooding attribution studies in the US were published between NCA4 and NCA5 (see section 6.6.3). In NCA5:

> "Many other extremes, including heavy precipitation, drought, flooding, wildfire, and hurricanes, are becoming more frequent and/or severe, with a cascade of effects in every part of the country" (NCA5, p.1-16).

Comment 4.

Page Number in DOE CWG Report: 68
Statement in DOE CWG Report:

> "The absence of detectable US-wide trends in flooding is consistent with the findings in Section 6.4 of absence of coherent changes in extreme precipitation."

Comment type: technical

Comment: There have indeed been detectable trends in flooding across the US (see comment 3 above), stemming from (a) increased extreme precipitation, (b) earlier snowmelt, and (c) storm surges. Climate change has contributed to some of these flooding trends and events (see

section 6.6.3). There is confidence in trends in increased extreme precipitation for certain regions of the US (see references in section 6.6.3).

6.6.3 Synthesis of Literature Based on Best Available Science

Climate change can exacerbate flooding in different ways, including via sea level rise and storm surges, hurricanes, rapid and early snowmelt, and heavy precipitation events.

While sea level rise is discussed in a different section (Chapter 7), it is relevant for flooding in coastal regions. Sea level rise can contribute to flooding when coupled with storm surges and high tide events (e.g., Sweet et al. 2018; Wdowinski et al. 2016; Gori et al. 2022). Furthermore, there has been an increase in hurricane-related damages over time (Grinsted et al. 2019), and hurricanes have started to reach further inland and move more slowly (Li and Chakrabotry 2020). Flooding from Hurricane Harvey is shown to be partially attributable to climate change (Wehner and Sampson 2021; Smiley et al. 2022).

Furthermore, in mountainous regions of the western US, climate change leads to a shift from snow to rain, earlier snowmelt, and rain-on snow events (Marvel et al. 2021; Lynn et al. 2020; McCabe and Clark 2005; McEvoy and Hatchett 2023), all of which can lead to flooding (e.g., Henn et al. 2020).

Finally, extreme precipitation can lead to both localized and downstream flooding. Climate change leads to warmer air temperatures, and warmer air can hold more water vapor — this principle has both exacerbated droughts (see drought response, section 6.7) and contributed to heavier precipitation events (Swain et al. 2025; Mallakpour and Villarini 2015).

There has been an observed intensification of extreme precipitation globally and across CONUS, largely due to more heavy precipitation events (NCA5; Kunkel et al. 2020; Dunn et al. 2020; Papalexiou and Montanari 2019), with flooding downstream (Mallakpour and Villarini 2015), and climate change contributed to this intensification (Diffenbaugh et al. 2017, 2018; Kirchmeier-Young and Zhang 2020; Davenport et al. 2021; Madakumbura et al. 2021). Note that while atmospheric river intensification is projected for California, it is not yet detectable (Williams et al. 2024), as it is currently masked by low precipitation due to natural variability and aerosols (see response to drought section, 6.7). With that said, the intensity of the atmospheric rivers that contributed to the Oroville dam flooding have been partially attributed to climate change (Michaelis et al. 2022).

There is also a projection of further future intensification of atmospheric rivers (Espinoza et al. 2018; Huang et al. 2020; Payne et al. 2020; Baek and Lora 2021) and overall flood risk (Wahl et al. 2015; Wing et al. 2022), including megaflood risk in California (Huang and Swain 2022).

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Section 6.7: Drought

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6.7.1 Summary

The drought section of the DOE CWG report argued that there has been no observed trend in frequency or intensity of 'drought'. However, this section is misleading. It often relies on a narrow definition of drought (conflating meteorological drought with other types of drought). It furthermore cherrypicks studies and quotes, relying on an outdated synthesis report, quoting sections out of context, and only including *one* peer reviewed study, while failing to reference the abundance of other studies from the rest of the peer-reviewed literature on this subject.

Contrary to what is stated in the DOE CWG report, there has been a proliferation of detection and attribution studies on how climate change has exacerbated drought, both in the US and globally. Many of these studies have been published in the past five years. These studies identify a significant climate change fingerprint on droughts over the past decades, and project more intense and longer duration droughts in the future. Particularly relevant to DOE is the impact of drought on energy demand which is increasingly affected by warmer surface temperature and high atmospheric evaporative demand (AED) over the Western US (e.g., Byers et al. 2020; Turner et al. 2019; Qiu et al. 2023).

The organization of this section is as follows: Section 6.7.2 reviews what the DOE CWG report did say followed by comments on what was not correct, left out, or otherwise lacking and what is needed to reconcile. Then section 6.7.3 summarizes best available science that the DOE report should have included in order for it to be a complete and unbiased summary of observations and projected climate change impacts on drought.

6.7.2 Addressing Each Statement in the DOE CWG Report

Comment 1.

Page number in DOE CWG Report: 68
Statement in DOE CWG Report: "Assessments of drought trends were as follows."
Comment type: Reference

Comment: This section relies fully on two IPCC-based reports, but takes comments out of context (see comments below), and one out-of-date National Climate Assessment (NCA4, USGCRP 2017) and similarly takes statements out of context (see comments before). However, it does not reference the proliferation of peer-reviewed detection and attribution studies nor projection-based studies that look at drought and climate change (see Section 6.7.3).

Action: The DOE CWG Report authors should edit this assessment by including the Fifth National Climate Assessment (NCA5, USGCRP 2023), providing an *accurate* representation of main-level findings in AR6 (IPCC 2021), SREX (IPCC 2012), and NCA5, and include the peer reviewed studies listed in Section 6.7.3.

Comment 2.

Page Number in DOE CWG Report: 68
Statement in DOE CWG Report: “AR6: Few AR6 regions show observed increases in meteorological drought (Section 11.9, p. 1575)”
Comment type: Technical

Comment: “Meteorological drought” is measured only using precipitation, but does not take into account other changes (e.g. in temperature or evaporative demand) which can lead to or exacerbate drought (see Section 6.7.3). Thus, relying solely on measures of "meteorological drought” to ascertain whether or not drought has become worse is misleading. The DOE CWG report authors would have been aware of this, because in the reference they use here (AR6), it makes this point:

> “The assessment is subdivided in three drought categories corresponding to four drought types: i) meteorological droughts, ii) agricultural and ecological droughts, and iii) hydrological droughts… [M]eteorological droughts are assessed based on observed and projected changes in precipitation-only metrics such as the Standardized Precipitation Index (SPI) and Consecutive Dry Days (CDD)” (IPCC AR6 2021, Section 11.9.4, p. 1605).

Action: The DOE CWG report authors should add what AR6 (IPCC 2021) states about agricultural, ecological, and hydrological droughts, as well as the influences of surface temperature, in addition to that of precipitation, on these droughts.

Comment 3.

Page Number in DOE CWG Report: 68
Statement in DOE CWG Report: “NCA4: As a consequence of this increased precipitation, drought statistics over the entire CONUS have declined.”
Comment type: technical & reference

Comment: First, it is misleading to look at spatial averages over all of CONUS when examining drought. While it is scientifically accurate to take large spatial averages of certain variables (e.g.,

global mean temperature, global atmospheric CO2 concentrations, etc), drought is an inherently local and regional phenomenon, as evidenced by the different trends observed in the Palmer Drought Severity Index (PDSI) during the last 50 years over the western US (WUS) and eastern US (EUS) (Figure 1). PDSI is a commonly used measure of drought (see section 6.7.3). With the observed increase in intensity of drought over the WUS, and with the increase in extreme flooding and precipitation events over the EUS (see response to Section 6.6), taking an average across the CONUS runs the risk of averaging out these trends (see Section 6.7.3). Indeed, effects of these droughts have been seen in the WUS, especially with groundwater running dry in part due to climate change (Liu et al. 2022; Chandanpurkar et al. 2025; see section 6.7.3).

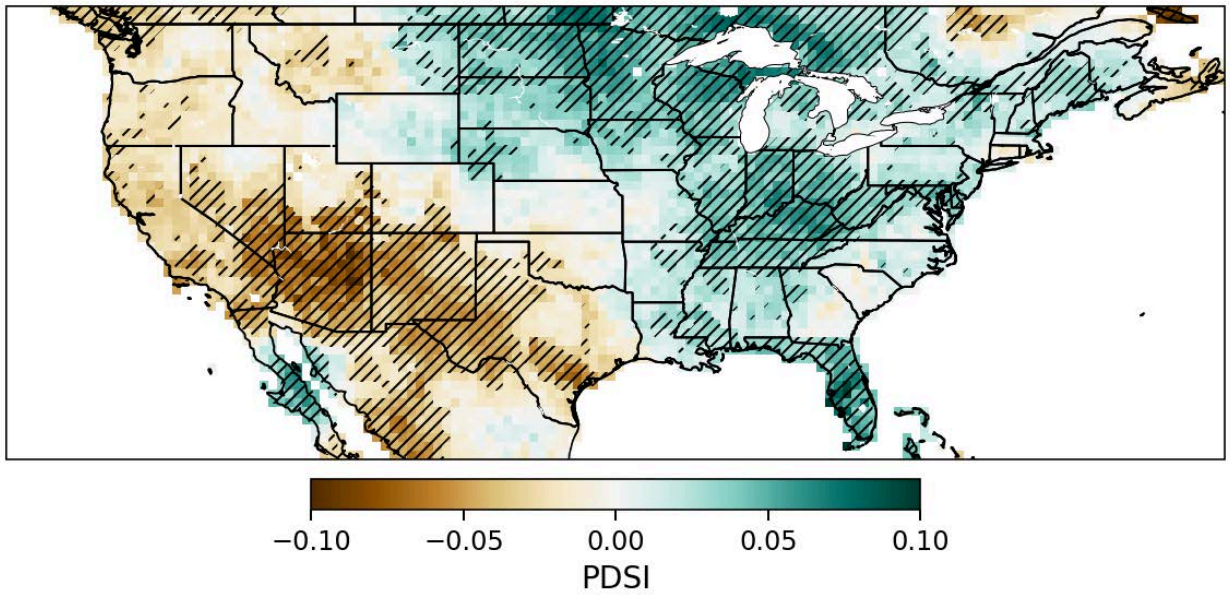

Figure 1. Annual self-calibrated Palmer Drought Severity Index trends from 1974-2023 (Barichivich et al. 2024). Trends were computed using a Theil-Sen estimator. Areas hatched are statistically significant based on Kendall's τ statistic ($p < 0.05$).

Second, this quote is taken out of context, as that section from NCA4 (USGCRP 2017) explains how climate change can exacerbate droughts in the US (DOE CWG report quote bolded):

> "Drought is, of course, directly connected to seasonal precipitation totals. Figure 7.1 shows detectable observed recent changes in seasonal precipitation. In fact, the increases in summer and fall precipitation are at odds with the projections in Figure 7.5. **As a consequence of this increased precipitation, drought statistics over the entire CONUS have declined**[3,12]. Furthermore, there is no detectable change in meteorological drought at the global scale. However, a number of individual event attribution studies suggest that if a drought occurs, anthropogenic temperature increase can exacerbate soil moisture deficits… [T]able 8.1 summarizes available attribution statements for recent extreme U.S. droughts. Statements about meteorological droughts are decidedly mixed, revealing the complexities in interpreting the low tail of the distribution of precipitation. Statements about agricultural drought consistently maintain a human influence if only surface soil moisture measures are considered."

Third, as this statement is largely in context of, again, *meteorological* drought, it would make sense to examine figure 7.1 in NCA4 (USGCRP 2017). Figure 7.1 in NCA4 is depicting observed changes in precipitation over the United States between the 'early period' (1901-1960) versus 'present day' (1986-2015) (USGCRP 2017, p. 209). Notably, while the figure shows increases in precipitation for some regions of the country, for much of the WUS, in the seasons where most precipitation falls (late fall, winter, and early spring), there are declines in precipitation. See Section 6.7.3 for a description of how observed *decreases* in precipitation over the southwestern US are linked to a combination of natural variability and aerosol-driven changes.

Finally, this reference is outdated. While the 4th National Climate Assessment (NCA4) represented the best available science at its time of publication in 2017, it was not the most recent national climate synthesis report available when the DOE CWG report was written, as the 5th NCA (USGCRP 2023) was published in 2023.

Action: To address this comment, the DOE CWG report authors should reference what the *5th* National Climate Assessment (NCA5, USGCRP 2023) has to say about drought instead of the 4th, and moreover provide a review of what NCA5 stated about each type of drought (meteorological, agricultural and ecological, and hydrological).

Comment 4.

Page Number in DOE CWG Report: 68
Statements in DOE CWG Report:

> "NCA4: Recent droughts and associated heat waves have reached record intensity in some regions of the United States; however, by geographical scale and duration, the Dust Bowl era of the 1930s remains the benchmark drought and extreme heat event in the historical record (very high confidence)."
>
> "SREX (IPCC 2012): From a paleoclimate perspective, recent droughts are not unprecedented, with severe 'megadroughts' reported in the paleoclimatic record for Europe, North America, and Australia. (p. 170)"

Comment type: technical

Comment: First, just because there have been more severe droughts in the historical record does not mean that climate change did not make recent droughts worse. A different 'flavor' of droughts now develop that are exacerbated by climate change, largely due to increased atmospheric evaporative demand (AED) (see Section 6.7.3; e.g., Zhuang et al. 2024; Williams et al. 2020; Weiss et al. 2009). The inverse is also true: the fact that climate change is making droughts worse does not mean that today's droughts are worse than any in the historical record.

Second, just because there have been more severe droughts in the historical record does not undo the fact that recent droughts have had profound consequences (Smith 2020).

Third, research has indicated that recent droughts in the WUS were more severe than droughts over the past 1000+ years: while megadroughts have occurred in the paleoclimatic record, the western US megadrought of 2000-2018 was the worst since the mid-1500 (Williams et al. 2020) and from 2000-2021 was the worst since 800 (Williams et al. 2022) as defined using soil moisture anomalies. Similarly, climate change made the 2012-2014 period in CA the driest period in 1200 years (Griffin and Anchukaitis 2014; Williams et al. 2015).

Finally, climate projections indicate that for regions plagued by drought (e.g., western US), future droughts will be even more severe (see Section 6.7.3; e.g., Naumann et al. 2018; Gebrechorkos et al. 2025; McEvoy et al. 2020; He et al. 2021).

Action: The DOE CWG report authors should describe how recent droughts of the past few decades compare to droughts in the historical record, specifically explaining the balance of AED-driven versus precipitation-driven droughts, include projections of western US drought for later this century, and at least mention the consequences of these droughts.

Comment 5.

Page Number in DOE CWG Report: 68
Statement in DOE CWG Report: “As shown in Figure 6.7.1, U.S. long-term data shows an insignificantly declining trend in extreme dryness (-0.001 percent per year)”
Comment type: technical

Comment: Figure 6.7.1 in the DOE CWG Report depicts the percent of CONUS that is “very dry”, defined as being < 10th percentile for precipitation. There are three scientific inaccuracies in relying on this figure to interpret a presence or absence of drought trends in the US:

1. It is misleading to look at an averaged value of any drought metric over all of CONUS, because there are different patterns across the region, with generally more intense precipitation observed in the eastern US with more drought in the western US (Figure 1; Section 6.7.3).
2. This figure is only based on precipitation. If ‘very dry’ instead was measured by soil moisture, streamflow, or other similar indicators, the figure would look different (e.g., Williams et al. 2020, 2022; Williams et al. 2024; Diffenbaugh et al. 2015; Gebrechorkos et al. 2025).
3. The trend line is based on monthly data from 1895-2025. However, this is misleading because it ignores recent trends in regional, AED-driven droughts that have been exacerbated by climate change (e.g., over Southwestern North America; Cook et al. 2021; Williams et al. 2020, 2022).

Action: To address this comment, the DOE CWG report authors should provide a spatial analysis of trends (e.g., Figure 1) or separate time series for different regions of the US (regions used by the US Drought Monitor: https://droughtmonitor.unl.edu/CurrentMap.aspx). Furthermore, the authors should present any analysis of drought using a well-accepted metric

(e.g., SPEI or PDSI), or at the very least a time series of AED (ideally of VPD or reference evapotranspiration) in addition to precipitation.

Comment 6.

Page Number in DOE CWG Report: 69
Statement in DOE CWG Report: “Kogan et al. (2020) examines a 38-year high-resolution satellite-based drought measure and concludes that global drought has not intensified and is not connected to climate change: “it is possible to state firmly that global and main grain countries’ drought area and intensity trends have not been following global climate warming since 1980’s.””
Comment type: technical & reference

Comment:

There are a plethora of different studies that conclude that droughts *have* intensified around the world (Zeng et al. 2023; Vicente-Serrano et al. 2022) and that this intensification is at least in part due to anthropogenic climate change (e.g, Gebrechorkos et al. 2025). Importantly, rather than the entire globe uniformly having an increase in drought, there are instead regional patterns where for some regions droughts have become more intense while for others flooding has become more intense (see Sections 6.6 and 6.7.3). Moreover, other studies have shown that drought has intensified over the western US due to climate change (Williams et al. 2020,2022; see Section 6.7.3). All of these studies were available at the time of writing the DOE CWG Report.

Furthermore, the approach by Kogan et al. (2020) in taking spatial averages across hemispheres and countries, unfortunately averages out the drought trends that have occurred. Kogan et al. (2020) looked at trends by hemisphere and by country, which, as expected, would suggest little change in drought due to the spatial averaging across regions with strongly different patterns in drought indices (see Section 6.7.3 for problems in taking spatial averages over CONUS for interpreting drought). Indeed, a more recent study that, like Kogan et al. (2020), also looks at a satellite-based vegetation health index for use in drought monitoring (Zeng et al. 2023) identifies how certain areas of the world experience increased drought while others experience decreased. Regions experiencing increased drought include the western US, while the southeastern US shows some improvement.

Action: To address this comment, the DOE CWG report authors should provide a comprehensive and unbiased review of peer-reviewed studies that have examined climate change and observed drought, particularly studies on the US, in order to respond to the mandate of this report. Please see Section 6.7.3 for a more comprehensive literature review.

Comment 7.

Page Number in DOE CWG Report: 69
Statement in DOE CWG Report: “In summary there is no evidence of increasing meteorological drought frequency or intensity in the U.S. or globally over recent decades.”
Comment type: technical

Comment: First and foremost, this statement is misleading. For a section about drought, the focus should be on drought as a whole, not just meteorological drought. As explained in Section 6.7.3, there is both evidence of increasing frequency and severity of droughts of different types in the US and globally, and evidence that some of this increase is due to anthropogenic climate change. This is moreover misleading as earlier in this section in the DOE CWG report (p.68), the authors quote the following from AR6 (IPCC 2021), which is missing from their “in summary” statement:

> “AR6: Increasing trends in agricultural and ecological droughts have been observed on all continents (medium confidence), but decreases only in one AR6 region (medium confidence). Increasing trends in hydrological droughts have been observed in a few AR6 regions.”

The statement is also inaccurate as it is not reflective of the best available science which was available at the time of writing of the DOE CWG report. In the WUS, while natural variability explains much of the low precipitation (i.e. meteorological drought) over the past few decades, recent studies suggest that aerosol emissions (emissions associated with fossil fuel combustion in lieu of scrubbers on smokestacks) explain some of the decrease (Kuo et al. 2023; Risser et al 2024).

Action: Given that this statement is likely intended to provide a summary of this section, the DOE CWG report authors should revise this statement to:

> “In summary, while there is some limited evidence that climate change has contributed to meteorological drought intensity over the western US, there is strong evidence that climate change has contributed to hydrological, ecological, and agricultural drought in the western US and other regions around the world over the past few decades.”

Comment 8.

Page number in DOE CWG Report: 69-70
Statement in DOE CWG Report: The DOE CWG Report is largely missing a discussion of changing aridity and desertification, which is highly relevant for both drought (this section) as well as for natural and managed ecosystems. There is a single mention on Page 6, section 2.1.3 where the authors assert that aridification of drylands is not an issue, based on a single study by Zhang et al. (2024) that notes due to increased CO_2 levels “increasing aridity in drylands won’t lead to a general loss of vegetation productivity”.

Comment type: technical

Comment: Studies have shown that, globally, climate change has contributed to the desertification of drylands (Burrell et al. 2020) and expansion of dryland (i.e. conversion of humid to dryland area) (Koppa et al. 2024). Other studies project this will continue, with future aridification and dryland expansion projected particularly for regions that are already semi-arid (Huang et al. 2016), including the Mediterranean and North African Regions (Carvalho et al. 2022; Lickley and Solomon 2018; Seager et al. 2014) and the western US (Lickley and Solomon 2018; Udall and Overpeck 2020). This has important implications for drought and vegetation productivity (e.g., Cao et al. 2022). Regarding the response of vegetation productivity to aridification and dryland expansion, please see response to Section 2.1.

Action: We recommend that the report authors summarize research on climate change and broader aridification and desertification in order to better present the current state of knowledge, and outline the existing areas of uncertainty.

6.7.3 Synthesis of Literature Based on Best Available Science

Definitions of and Contributors to Drought

There are a number of different ways to define “drought”, but the most agreed upon way is simply insufficient supply of water to meet demand (USGCRP 2023; Cook et al. 2018). A more specific definition depends on what system you are studying: for example, drought might be defined by insufficient surface water (e.g., in rivers) to meet local demand of cities, or it might be defined by insufficient soil moisture to meet demand of plants in an agricultural land, grassland, or a forest.

There are several definitions of drought which are most commonly used:

- Meteorological drought: Defined as abnormally low precipitation, and measured with indices designed to just look at precipitation (e.g., SPI) (AR6, IPCC 2021, Section 11.9.4, p.1605). *Note, the drought section of the DOE CWG report relied largely on this definition.*
- Agricultural and ecological drought: Defined as insufficient water to meet agricultural or ecological demand (e.g., insufficient water for irrigation, insufficient soil moisture for forests or grasslands, etc.), and often based on measures of soil moisture, water balance (precipitation minus evapotranspiration), and indices designed to measure both (e.g., SPEI, PDSI) (AR6, IPCC 2021, Section 11.9.4, p.1605).
- Hydrological drought: Defined as abnormally low surface water flow and changes in runoff (AR6, IPCC 2021, Section 11.9.4, p.1606).

Therefore, depending on *how* you define drought and what measures you use, you will get different answers to whether drought frequency or severity has changed, whether that change is partially due to climate change, and how it might change in the future (Cook et al. 2018; Mukherjee et al. 2018; Vicente-Serrano et al. 2025). While meteorological drought, by definition, is only influenced by precipitation, agricultural and ecological drought and hydrological drought

are influenced by precipitation, atmospheric evaporative demand (AED), and a number of other complex land surface and even social processes. AED is the evaporative potential of the air (i.e. how “thirsty” the air is) which is a function of temperature and humidity, and AED has increased in many regions of the world, including in the US, largely due to climate change (Albano et al. 2022; Williams et al. 2020, 2022; Williams and Abatzoglou 2025; Gebrechorkos et al. 2025; Vicente-Serrano et al. 2025; Marvel et al. 2025).

Climate Change and Observed and Projected Changes in Drought in the US

Droughts (and flooding, Section 6.6) happen at local to regional scales, and similarly, trends in both are locally- and regionally-specific. Climate change has affected the whole hydrologic cycle, where in certain regions droughts are now more severe and frequent, while in others large flooding events are happening with more frequency and severity. In the US, the *broad, general* trend is that the western US (WUS) has had more droughts, while the eastern US (EUS) has had some drought improvement as well as more floods (with important caveats to each) (Zeng et al. 2023; IPCC AR6 SPM 2021; Herrera et al. 2023; USGCRP 2023).

Western US (WUS)

In the western US (WUS), droughts are geographically larger, longer, more likely, and more intense, partially due to climate-change-driven increases in AED (A.P. Williams et al. 2020, 2022; E.L. Williams et al. 2024, 2025; Zhuang et al. 2021; Zhuang et al. 2024; Diffenbaugh et al. 2015). Moreover, studies suggest that the low precipitation observed over the WUS over most of the past two decades was due to a combination of natural variability and human activities that contribute to climate change: specifically the aerosol emissions (e.g., sulfur dioxide) that have accompanied the burning of fossil fuels (Lehner et al. 2018; Kuo et al 2023; Risser et al. 2024; Chiang et al. 2021). Therefore, due to climate change, there has been an increased likelihood of hot conditions (and therefore high AED) co-occurring with dry conditions (i.e. low precipitation) (Diffenbaugh et al. 2015; Alizadeh et al. 2020; A.P. Williams et al. 2015).

Due to these mechanisms, climate change has made different types of drought worse in the WUS, and is projected to keep doing so.

Ecological drought. Studies have demonstrated that climate change exacerbated the megadrought of the past two decades (Williams et al. 2020, 2022), resulted in droughts that are more driven by AED than precipitation declines (Zhuang et al. 2024), made the 2012-2014 period in CA the driest period in 1200 years (Griffin and Anchukaitis 2014; Williams et al. 2015), and made it harder to recover from drought once in one (Williams et al. 2024). Climate change is projected to increase the likelihood of extreme multi-year droughts (Cook et al. 2021; McEvoy et al., 2020) and a response in vegetation productivity in response to those droughts (Cao et al.

2022) and the timing of key variables for ecological drought (Marvel et al. 2021), due to increase in atmospheric evaporative demand over the WUS (Gamelin et al. 2022; Ficklin and Novick 2017). Further, projected groundwater declines are anticipated to add to ecological drought risks in high elevation forests (Carroll et al. 2024).

Hydrologic drought. Studies that use measures based on ecological drought are relevant for hydrologic drought as well. In addition, as WUS hydrology is heavily dependent on snowpack and snowmelt (Hale et al. 2023), studies on these variables are also relevant. There has been an observed and projected increase in snow droughts (Cowherd et al. 2023; Huning and AghaKouchak 2020), in part due to a change in precipitation partitioning (i.e. less snow and more rain) (Lynn et al. 2020), with a decrease in overall snow water storage in the WUS (Hale et al. 2023). Studies have shown that climate change has contributed to snow droughts via less snowpack and earlier snowmelt runoff (Gottlieb and Mankin 2024; Shukla et al. 2015; Mote et al., 2018). There are projected future reductions in snowpack, especially later season (April) snowpack which is important for the summer water budget (Siirla-Woodburn et al., 2023; Norris et al., 2025; Marvel et al. 2021). In CA's Central Valley (hosting much of the nation's agriculture), there is a projected continued shift toward earlier streamflow timing, meaning less water available during the hot, dry season (i.e. summer) in the WUS (He et al. 2021). Similarly, groundwater declines in the Upper Colorado Basin are projected to substantially (12-27%) reduce downstream waterflow in addition to projected streamflow loss not accounting for reduced groundwater input (21-81%)(Carroll et al. 2024). Snow-dominated watersheds in the Upper Colorado Basin, Wyoming, and Utah provides nearly 90% of the water to the Colorado River (Jacobs 2011), which supports 40 million people and US$1.4 trillion in economic activity (James 2014).

Agricultural drought. Climate change has increased the risk of agricultural drought, increasing irrigation demand (or the amount of irrigation that would need to be applied to a field to match AED) (Williams and Abatzoglou 2025) and disrupting snowmelt (see 'hydrologic drought' above), which is a key water supply for agriculture (Qin et al. 2020). Water necessary for irrigation for agriculture will be even further stressed, as WUS agriculture relies heavily on snowmelt (Qin et al., 2020). This has already been observed, with groundwater levels in both California and the WUS in rapid decline (Liu et al. 2022; Chandanpurkar et al. 2025; Perrone and Jasechko 2017; Jasechko and Perrone 2021).

Hydroclimate whiplash. While the WUS has historically had large interannual hydroclimate variability (e.g., Cook et al. 2025), research has indicated that climate change has already increased this variability ("hydroclimate whiplash"; Swain et al. 2025), meaning more dramatic swings from very wet to very dry years, with implications for both drought *and* flooding. Climate change is projected to increase hydroclimate whiplash even more in the future (Swain et al. 2018; Chen et al. 2022).

Eastern US (EUS)

While there is scientific consensus that climate change has increased drought in the WUS, in the eastern US (EUS), this is not the case (IPCC AR6 SPM 2021, p.10). Although there has been an increase in AED over the EUS due to climate change, this has been masked by changes in precipitation (Williams and Abatzoglou 2025).

Climate Change and Global Observed and Projected Changes in Drought

Globally, there have been more severe droughts due to increased AED (Vicente-Serrano et al. 2025; Trenberth et al. 2014; Gebrechorkos et al. 2025; Vicente-Serrano et al. 2022; Marvel et al. 2025; Li et al. 2024). Moreover, there has been an increase in “flash droughts”, which are droughts that onset or intensify rapidly (Yuan et al. 2023; Christian et al. 2021).

Importantly, this does not mean that *everywhere* has had more droughts, but instead that there has been an increase in drought intensity in many areas around the world. This is, again, because certain areas have both observed and projected intensification of drought due to climate change, other areas do not (or even have the opposite, an increased wetting) (Naumann et al. 2018; Cook et al. 2018; Zeng et al. 2023; Gebrechorkos et al. 2025; Chiang et al. 2021).

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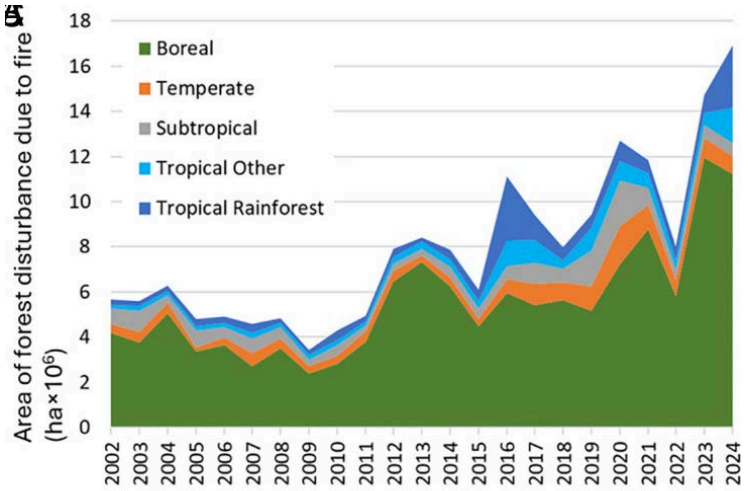
Comment on Section 6.8, "Extreme weather: Wildfires"

John M. Wallace, Chan-Pang Ng, David Battisti, and Qiang Fu

Section 6.8 (*p. 69-71*) Wildfires

The discussion in this section relates to forest fires, yet the leadoff graphic (*Fig. 6.8.1*) relates to globally integrated wildfire area, which is dominated by grassland fires set by farmers to clear croplands in the Sahel and its Southern Hemisphere counterpart. The area burned per year has indeed been slowly decreasing in response to improved agricultural practices. While that's good news, it's irrelevant to this section, because the global time series shown in this figure bears little relation to the area of forest burned by wildfires. Figure 1A of Potapov et al. (2025) based on forested areas, reproduced here, conveys an entirely different impression.



The statement "*Global data show that wildfire coverage is constant or declining on every continent (Samborska & Ritchie, 2024)*" on p. 69 is likewise deceptive, because the graphic relates to all wildfires, including those in grasslands and croplands.[1]

Citing Cunningham et al. (2024) and Tyukavina et al. (2022), the authors of the DOE Report acknowledge that (in their words), "wildfires are increasing in some parts of the world". However, they don't mention that the increases greatly outweigh the decreases, and that the most heavily impacted areas (i.e., the boreal forests and the forests of western US) have experienced a doubling or more of the per year fire-related forest loss over the past 25 years. Nor do they make any mention of the plethora of evidence presented in the literature attributing the worsening wildfire crisis to warming induced by the burning of fossil fuels.

[1] The reference is to the *Our World in Data website* (https://ourworldindata.org/forests-and-deforestation), on which a wildfire is defined as "an uncontrolled burn of vegetation, which includes the burning of forests, shrublands and grasslands, savannas, and croplands."

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Section 6.8: Wildfire

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Summary

The wildfire section of the Department of Energy Climate Working Group (DOE CWG) report focuses on the global decrease in burned area, and trends in US wildfire over the last 1400 and 100 years. The global decline in burned area is driven by land use change in African grasslands, which is not related to greenhouse gas emission impacts, whilst burned area in forested regions – including the US – has increased in recent decades. The subsequent focus on the 'wildfire deficit' confounds climate change with changing management practices in the same period. These two lines of investigation are overly specific, do not represent important changes to wildfire in the US or globally, and do not address the extensive scientific literature on the impacts of climate change on wildfire. The likelihood and intensity of the most extreme wildfires has increased in North America, which has had major effects in the western US. These extreme fires have generated substantial ecological, economic, health, and livelihood impacts across the US. The consequences extend far beyond fire-prone regions due to smoke transport across continental distances, exposing populations nationwide to serious health risks. Given both the expanding scope of wildfire impacts and growing public awareness, this topic deserves a clearer and more robust scientific treatment. In the following section, we review each statement in the DOE CWG report and provide corrections, additions, and suggestions for improving the report.

Addressing Each Statement in the DOE CWG Report

Page: Overall comment on Section 6.8, with implications for the entire report.
Comment Type: Technical

Comment: Typically scientific assessments aim to assess the current state of scientific knowledge based on a synthesis of recent scientific literature. Often these assessments use standards to determine the level of agreement/disagreement in the scientific literature and the level of confidence in conclusions that can be drawn therefrom. For example,

Intergovernmental Panel on Climate Change (IPCC) authors assess whether there is limited, moderate, or robust evidence for a scientific conclusion based on the availability of data and papers on the issue. Likewise, they assess the confidence that can be placed on a conclusion based on the volume of evidence and the number of lines of evidence that converge on the conclusion. This section, and most of this report, does not present an assessment of the current state of science. Rather,the authors are making a statement of position and supporting it with selective citations of literature, often with misleading statements. A large body of peer reviewed literature is ignored or key messages from cited papers are not included in the text. For the remainder of the review of this section, we will assume that the authors need to answer the question of how climate change affects wildfire and the impacts of these on human wellbeing.

Comment 1
Page: 69, Paragraph 1 Section 6.8
Comment Type: Technical
Statement: "The IPCC has not provided an attribution assessment of wildfires"

Comment:

Factual errors: While there is no explicit section just on wildfire in the physical science basis 6th assessment of the IPCC report (AR6) (IPCC 2021), there is a review of how climate change has exacerbated fire weather and led to increased burned area (AR6 WGI, Section 11.8.3, p. 1600). Furthermore, there is a section describing how fire weather conditions have increased in the western US, and that this increase is largely due to climate change (AR6 WGI, Section 12.4.6.2, p, 1832). These are both attribution statements. Furthermore, it describes how future climate change is projected to lead to even greater fire weather (AR6 WGI, Section 12.4.6.2, p, 1832). Also, the cross-chapter box #3 in the Special Report on Climate Change and Land (which was part of the AR6 cycle) discusses fire trends and notes the recent increase in W. North America (IPCC 2019). The DOE CWG Report authors need to fix this statement to accurately reflect what the IPCC reports have said about climate change and wildfire.

Since AR6, there have been a considerable number of attribution studies on the response of extreme fire weather to climate change. Since AR6, peer-reviewed studies have demonstrated that climate change has led to increased fire weather (Abatzoglou et al. 2025; Jones et al. 2022, 2024, Turco et al. 2023; Balch et al. 2022; Zhuang et al. 2021; Goss et al. 2020), longer fire seasons (Madakumbra et al. 2025; Swain et al. 2023), and drier fuels (Alizadeh et al. 2021; Ellis et al. 2021; Goodwin et al. 2021). Climate change has led to greater burned area in forests (Abatzoglou et al. 2025; Jones et al. 2022, 2024, Turco et al. 2023) and increased PM2.5 pollution (Burke et al. 2023; Law et al. 2025; Chen et al., 2025) and GHG emissions from fires (Zheng et al. 2021, 2023), and contributed to more intense fires, including the 2025 Los Angeles fires (Barnes et al. 2025).

Action: The DOE CWG Report authors should revise their statement to accurately reflect what the IPCC has said about climate change and wildfires, and include new information from the

above studies in their evaluation of the state of the science regarding climate change and wildfires.

Comment 2
Page 69 Section 6.8 Paragraph 1 and Figure 6.8.1
Comment Type: Technical
Statement: “As shown in Figure 6.8.1, global wildfire activity as measured by the European Space Agency shows a downward trend in the 21st century...Global data show that wildfire coverage is constant or declining on every continent (Samborska and Ritchie, 2024).”

Comment:

Though there has been a decrease globally in fires in savannas (associated with land use change and landscape fragmentation; Andela et al., 2017; Bowman 2020; Zubkova et al., 2023; Jones et al. 2022; Harrison et al., 2025), both burned area and fire carbon emissions have *increased* globally in forested regions (Jones et al. 2022; Jones et al. 2024; Zheng et al. 2021). There has been a large increase in fire activity in the western US (e.g., Weber & Yadav, 2020), and studies have demonstrated that climate change led to an increase in burned area in forested regions of the western US (Abatzoglou and Williams 2016) and in California forests (Turco et al. 2023; Williams et al. 2019).

Action: The DOE CWG Report authors should highlight the clear increasing trends in extreme fire behaviour and fire weather in different regions -- especially in the US and in Canada (due to transported smoke). We would invite the authors to present a more nuanced assessment, as not all continents are exhibiting decreased fire counts or burned area, as observations (Marlon, 2020) and models (the Fire-MIP analysis, e.g. van Marle et al., 2017) both show, and the continental analysis hides, important regional trends.

Comment 3
Page 69 Section 6.8 Paragraph 2
Comment Type: Technical
Statement: “However there is evidence that the intensity of fires in some regions is worsening (Cunningham et al. 2024) and that wildfires resulted in a net loss of global forest cover over 2001-2019 (Tyukavina et al. 2022).”

Comment: This is the only place in which the DOE CWG report authors include attribution studies on wildfire. In addition to these studies, other studies have shown that there has been an increase in extreme fire risk and burned area due to climate change, including in the western US (see comment 1 for a list of these studies). In addition to those studies published since AR6 (again, see comment 1), a number of earlier studies also make this point (e.g., Abatzoglou and Williams 2016; Williams et al. 2019; Kirchmeier-Young et al. 2017; Westerling 2016; Flannigan

et al. 2009), mainly due to increase of surface temperature in spring and summer, and reduced snow cover.

Action: The DOE CWG Report authors should integrate these key studies and their main conclusions into the analysis (i.e. those listed here and in comment 1) for a less partial representation of the peer-reviewed science on climate change and fire.

Comment 4
Page: 69 Section 6.8 Paragraph 3
Comment Type: Technical
Statement: “Marlon et al. (2012) used sedimentary charcoal layers to reconstruct fire history of western U.S. for the past 1400 years and also fit a model to predict fire activity as a function of climatic conditions. Their results are summarized in Figure 6.8.2 below (from Figure 2 in their paper). There has been a growing wildfire deficit over the 20th century. In other words, however much fire was observed in the 20th century, it was less than what would have been observed in previous centuries based on the climatic conditions.”

Comment: The DOE CWG report's summary of Marlon et al. (2012) fundamentally misrepresents the paper's central conclusion regarding the 20th-century “fire deficit.” While the report correctly notes the existence of this deficit, it cherry-picks this single concept while ignoring the explicit causation discussed in the paper. The research clearly attributes the abrupt decline in burning since the late 1800s to the “combined effects of human activities, ecological, and climate changes,” with active fire suppression being a primary human driver. The paper explicitly states: “The divergence in fire and climate since the mid 1800s CE has created a fire deficit in the West that is jointly attributable to human activities and climate change, and unsustainable given the current trajectory of climate change.” In other words, while aggressive fire suppression has, over many decades, led to fuel build-up that contributes to more uncontrollable fires, increasing temperatures from rising greenhouse gas emissions are also exacerbating wildfires, increasing area burned, fire severity, and associated economic costs in the western US.

By stating the deficit arose because 20th-century fire activity was “less than what would have been observed…based on the climatic conditions,” the DOE report inverts the study’s finding. It incorrectly implies the climate was less fire-prone than in previous centuries, whereas the analysis shows that humans were both causing climate to become *more* fire prone and also directly suppressing fires *despite* these climatic factors; this combination has led to both a dangerous accumulation of fuel and a more fire-prone climate. The mischaracterization of the research in the DOE CWG report ignores a foundational, long-resolved argument in fire science: that the legacy of suppression has primed forests for more severe fires at the same time that humans are making the climate hotter. The report thus misuses the work to obscure the reality identified: this growing deficit, exacerbated by modern climate warming, is a key reason for the increase in large, high-severity fires today.

Action: The DOE CWG authors should fix this factual error and faithfully represent the findings of Marlon et al. (2012).

Comment 5
Page: 69
Comment Type: Technical
Statement: “Parks et al. (2025) likewise find that despite the recent increase in wildfire burn area in North America, a significant wildfire deficit remains relative to historical wildfire regimes.”

Comment: First, this is misleading as, of course, there is a fire deficit due to the decades of historical fire suppression, and the implication is that even with the climate-fueled increase in severity of fires, fire activity hasn’t yet reached pre-suppression levels (see Parks et al. 2025 Fig. 4). This misrepresents Parks et al. (2025). For instance, they end the abstract by saying: “...there is abundant evidence that unprecedented contemporary fire severity is driving forest loss in many ecosystems and adversely impacting human lives, infrastructure, and water supplies.”

With respect to wildfire deficit, it is true that there is currently still less fire than would be expected based on climate alone; this is due to successful fire suppression in recent decades. Nevertheless, contemporary fires burn at higher severity and intensity than those prior to western settlement. Most of the contemporary burned area occurs when fires evade fire suppression efforts, when fuels are critically dry, and during periods of heightened fire weather. There is widespread evidence of more frequent periods of high fire weather over the past half-century, driven by climate change, across the western US that contributed to the well documented increase in burned area in the western US, particularly in forested areas (Jones et al., 2022; Parks and Abatzoglou, 2020). The DOE CWG authors neglect the recent increase in fire severity and length of fire season in the western US that is largely driven by climate change. They fail to connect climatic changes to the likelihood that wildfire will continue to increase in the western US (and other parts of the globe) and they don’t mention the economic, health, ecosystem service, and ecological costs that we need to prepare for.

Action: The wildfire deficit concept should be accurately described within the broader context of climate change (as explained above) The DOE CWG authors should revise this paragraph to make it scientifically accurate and policy relevant. They can do this by demonstrating that the current trend of increasing wildfire severity is the result of previous fire suppression decisions that led to excessive fuel buildup coupled with increased intensity and duration of fire weather (due to climate change) that exacerbates fuel flammability. This would then support the discussion of wildfire vs. prescribed burning below.

Comment 6
Page: 70, Paragraph 1, Section 6.8 & page 71 Figure 6.8.3
Comment Type: Technical

Comment: The paragraph indicates that fires are not increasing in the US, which is misleading. This paragraph is discussing Figure 6.8.3. In that figure, the data before 1983 should not be compared to the data after 1983, as explained by the source website that data in the graph come from:

> "Prior to 1983, the federal wildland fire agencies did not track official wildfire data using current reporting processes. Statistics are subject to substantial changes in what was codified as a wildfire and burned area (e.g., inclusion of incendiary fires plague reporting issues prior to the 1960s, as well as duplicate counting of fires in federal records). As a result, there is no official data prior to 1983 posted on this site."

The authors also note inconsistencies prior to 1960 in the time series. Instead, looking at the data from 1983-onward (corresponding to when there was consistent data), there is evidence of increased burned area and increased number of very large fires. The section fails to mention that fire is increasing in extent and severity in several regions of the US (see comment 1), boreal North America, and the Arctic with economic impacts on Americans (see comment 7). For example, although the observed wildfire record is short in some regions, Fire Model Intercomparison Project (Fire-MIP) analyses show an increase in extreme events across the boreal region of North America and increasing CO_2 emissions from 1920 to the present day (van Marle et al., 2017). Similar results can be found in several other publications (van der Werf, et al. 2017; McCarthy et al., 2021, etc.). Therefore, a conclusion like "Nonetheless just focusing on the post-1985 interval the number of fires is not increasing" is misleading.

Action: Instead, the DOE CWG report authors should highlight the *robust evidence* that burned area, fire severity, and fire related emissions are increasing across the North American boreal zone, the Arctic, and in the western half of the US (Abatzoglou and Williams 2016; Turco et al. 2023; Williams et al. 2019; Flannigan et al. 2009; Zheng et al. 2023; Kirchmeier-Young et al 2017), and we can draw this conclusion from the scientific literature with *high confidence*. There is enough evidence in the scientific literature to further unpack this conclusion and talk about particulate matter (PM) with a diameter of 2.5 micrometers or less (PM 2.5), black carbon, and other pollutants (e.g., Burke et al. 2023; Law et al. 2025). The authors should include more information on trends in extreme fire driven by the increasing likelihood of extreme fire weather and to report on issues at both a continental and seasonal scale (see comment 1).

Comment 7
Page: 70, Section 6.8, Paragraph 2
Comment type: Technical
Statement: "Forest fires have always been a part of nature, and they can certainly create conditions that are inhospitable in the short term for all life, including humans. Science has confirmed the overall benefit and necessity of forest fires. While recent high-profile fires and seasons serve as a reminder of the potential destructive impact, the highest profile U.S. forest fire remains the 1910 Big Blowup fire in the U.S. west, which destroyed over three million acres and eliminated entire towns like Taft, MT (Apple, 2020). The 1910 fire reshaped the U.S. Forest Service (National Forest Foundation 2022) leading to a focus on fire suppression with a primary

goal of defeating all forest fires (Forest History Society, 2022). This led to the "10 am rule" in 1935 requiring that all fires spotted on any day had to be controlled by 10 am the following day (National Forest Foundation, 2022)."

Comment: This statement advances the simplistic assertion that "Science has confirmed the overall benefit and necessity of forest fires", while (1) ignoring the fact that fires today are more destructive than before—partially due to climate change—and (2) dismissing the very real impacts of modern fires on the American people.

The statement that "the highest profile U.S. forest fire remains the 1910 Big Blowup fire in the U.S. west" is highly subjective and unsupported. For example, over the past century in California alone, 18/20 of the most destructive fires (in terms of property lost), and 13/20 of the deadliest fires, have occurred since 2000 (CAL FIRE 2025a,b). Research has demonstrated that while part of this increase in destructive fires is due to more people living in the wildland-urban interface (WUI), coupled with a century of wildfire suppression leading to fuel build-up, climate change explains part of this increase in destructiveness. Specifically, climate change has led to elevated fire weather (Abatzoglou et al. 2025; Zhuang et al. 2021; Goss et al. 2020; Jones et al. 2022; Abatzoglou et al. 2019) and drier fuel (Ellis et al. 2021; Goodwin et al. 2021; Abatzoglou and Williams 2016; Alizadeh et al. 2021). Therefore, when fires ignite, they burn at higher intensity (Parks and Abatzoglou 2020). When drought-ridden forests experience these higher severity fires, this negatively impacts ecosystem recovery (Turner et al. 2019; Stevens-Rumann et al. 2018). Furthermore, regarding the 10 am rule, climate change has made it more difficult to control fires overnight, when most fire suppression efforts have historically happened (Balch et al. 2022).

The broad generalization ("Forest fires have always been a part of nature") neglects the human health and mortality, economic, and ecological tolls of high intensity and more frequent modern fires in some regions, such as the western US (Ostoja et al. 2023). First, recent fires (e.g., Lahaina, Maui; LA Eaton and Palisades fires; Paradise Fire) have led to hundreds of direct deaths. Furthermore, smoke exposure has increased morbidity and mortality among exposed populations (Law et al. 2025; Xu et al. 2020; Cascio 2018) and can lead to adverse pregnancy outcomes due to maternal exposure (Abdo et al. 2019).Wildfire smoke has "substantially slowed or reversed improvements in ambient PM 2.5 concentrations throughout much of the USA" (Burke et al. 2023). Smoke exposure also affects the eastern US, with high smoke-related mortality (O'dell et al. 2021) due to transported smoke from wildfires in Canada and the western US (Burke et al. 2021).

The economic cost (in terms of response and losses) of wildfires is tremendous and has been increasing over the last several decades (Bayham et al. 2022), with estimated federal response costs to fires in 2020 at ~$2 billion, damages from 2020 fires at ~$16.5 billion, and direct and indirect costs of 2018 fires in California at ~$148.5 billion (Bayham et al. 2022; Wang et al. 2021). Rising insurance costs from wildfires have also led to market withdrawals in high-risk states like California, leaving homeowners uninsured and financially vulnerable (Almuhana,

2024, Auer, 2024). Wildfires also destroy forest stands used for timber (Bousfield et al. 2025) and contaminate drinking water supply (Murphy et al. 2018; Paul et al. 2022).

Finally, modern wildfires also threaten the capacity of forests to act as critical carbon sinks while releasing carbon dioxide, contributing to the acceleration of climate change in a positive feedback (Jones et al. 2024; Zheng et al. 2021; Liu et al. 2014).

Action: Therefore, the DOE CWG Report authors should revise this statement to specify that while certain types of fires are beneficial (especially prescribed burns), recent fires (of the past decades) have been highly destructive, in part due to climate change. The DOE CWG Report authors should incorporate the points we make (above), including the literature we reference.

Comment 8

Page 70 Section 6.8 Paragraph 3
Comment Type: Technical Comment
Statement: "While defeating all fires seemed a noble goal, questions began to arise as to whether this behavior "followed the science" (U.S. Forest Service, 2022). Over time the U.S. Forest Service has begun to rethink its goals, recognizing that new approaches such as prescribed burns, fuel elimination, and controlled wildfires are more appropriate (Sommer, 2016). Recent research is validating this approach and recognizing that more frequent smaller fires likely result in healthier forests, water ecosystems and biodiversity (Stephens et al., 2021)."

Comment: While low- and moderate-severity fire can be ecologically beneficial and help to reduce fuels, large, high-severity fires often have extremely negative impacts (see comment 7). However, due to climate change, there is a narrower window for when prescribed burns (i.e. 'beneficial fire') can be safely conducted (Swain et al. 2023).

Action: The DOE CWG report authors should more clearly differentiate between beneficial, lower-severity fire and high-severity fire (not just smaller or larger fires), and note the challenges that historical fire suppression and climate change pose to conducting prescribed burns.

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Technical Comment on the DOE CWG report, Chapter 7: Changes in Sea Level

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The opinions, findings, and conclusions or recommendations expressed are those of the authors.

This section is a **technical** comment on chapter 7 of the DOE CWG report (pp. 75-81).

Chapter 7 of the DOE CWG report addresses the topic of changes in sea level. As the chapter correctly notes, global-mean sea level (GMSL) has risen by about 8 inches since 1900, and this rise is "unambiguously associated with increasing temperatures" (p. 75). The chapter omits three critical assessments, discussed below:

- It conducts no systematic, statistical analysis to support its claim that there is "no obvious acceleration" of sea-level rise, either at individual tide gauges in the United States or in aggregate across U.S. tide gauges.
- It conducts no analysis to support its claim that sea level is rising at a lower rate than projected by the Intergovernmental Panel on Climate Change (IPCC)'s Sixth Assessment Report (AR6).
- It contains no discussion of the current or future impacts of sea-level rise on the United States.

It also contains numerous statements that are not supported by a balanced assessment of the scientific literature, utilizes cherry picked data, and has pervasive issues with inadequate citation practices. Specific examples are discussed below (pp. 75-81).

To correct for these issues, the following changes must be made in the DOE CWG document:

- Update the text to correctly cite literature showing an acceleration of sea-level rise beyond the historical average rate, as measured by geological proxy records, tide gauges and satellite altimetry, and correctly describe this acceleration in the report executive summary.

- Include an explanation of which tide gauges are highlighted in the report and why, as well as a summary of the aggregate trends as reported in Sweet et al., 2022 and other sources
- Update the text to reflect the consistency between sea-level observations and the IPCC AR6 projections.
- Include a discussion of the numerous current and future impacts of sea-level rise on the United States, including the costs of private and public adaptation to sea-level rise.
- Clarify that the role of vertical land motion as a key driver of spatial variability in relative sea-level change has been long understood, and that subsidence exacerbates the risks associated with climate-driven sea-level rise.
- Correct the additional errors and misrepresentations identified below.

Critical failings

This is a **technical** comment on the summary of chapter 7 (p. 75) and its representation in the report executive summary (p. ix), as well as the text of the chapter that supports (or fails to support) these summaries (pp. 75-81).

A vibes-based assessment of sea-level acceleration

¶*1* of the chapter summary (p. 75) states that "U.S. tide gauge measurements reveal no obvious acceleration beyond the historical average rate of sea level rise". **The report executive summary (p. ix)** embellishes the chapter summary statement, further claiming that "U.S. tide gauge measurements *in aggregate* show no obvious acceleration in sea-level rise beyond the historical average rate [Chapter 7]" (emphasis added).

Despite making this claim, the report contains *no analysis* of "U.S. tide gauge measurements in aggregate." Whether or not acceleration is "obvious" to the authors of the DOE CWG report, Sweet et al. (2022) do analyze aggregate tide gauge measurements in the contiguous US (CONUS) and in regions of the United States and outlying islands. Their analysis, and follow-on Interagency analysis presented on the National Sea Level Explorer (earth.gov/sealevel/us), confirms sea-level acceleration since the 1970s in CONUS in aggregate, driven particularly by acceleration along the US Atlantic and Gulf Coasts (Figure 1). (The National Sea Level Explorer is a collaborative, interagency effort led by NASA, NOAA, and USGS.) Based on Interagency analysis, the CONUS aggregate rate of relative sea-level (RSL) rise has increased from 0.3 inches/decade (0.7 mm/yr) in the 1970s to 1.9 inches/decade (4.7 mm/yr) in the 2010s. Using a different statistical method, Piecuch (2025) demonstrates a sustained acceleration in CONUS sea level since 1900.

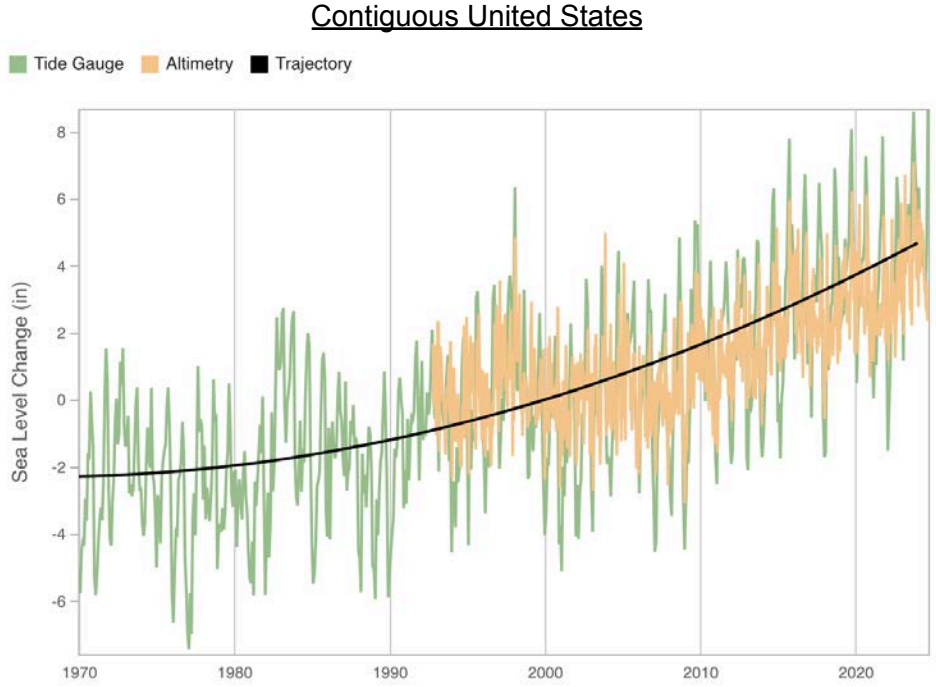

Figure 1. The aggregate tide-gauge and satellite altimetry record for the Contiguous US, showing clear acceleration from 0.3 inches/decade (0.7 mm/yr) in the 1970s to 1.9 inches/decade (4.7 mm/yr) in the 2010s. Source: https://earth.gov/sealevel/us/national-sea-level-explorer/?type=regional&region=USA&scope=section_1

The standard scientific approach for evaluating the presence of acceleration is through statistical analysis. The DOE CWG report, however, performs no statistical analysis to test for acceleration. Instead, it selectively shows graphs of linear fits to four selected tide U.S. tide gauges, and presents tabular results for these four gauges plus a fifth, unplotted tide gauge. It provides no rationale for how these five records were selected for presentation from among the 141 actively updated, long-duration NOAA tide gauges (https://tidesandcurrents.noaa.gov/sltrends/mslUSTrendsTable.html).

By contrast with the DOE CWG report's untested assumption of linearity, Interagency analysis shows that four of the five tide gauges the report focuses upon do indeed show an increase in the decadal rate of sea-level rise (i.e., an acceleration) of at least 1.5 inches/decade (3.8 mm/yr) between the 1970s and the 2010s (Figures 2-6). Note that the data for tide gauges and altimetry show a good correspondence (Figures 2-6) and changes in vertical land motion are taken into account (see below). The claims in the DOE report that there is no "obvious acceleration beyond the historical average rate of sea level rise" (p. ix and p. 75) are supported by the available empirical data and standard statistical analysis.

The Battery (New York City)

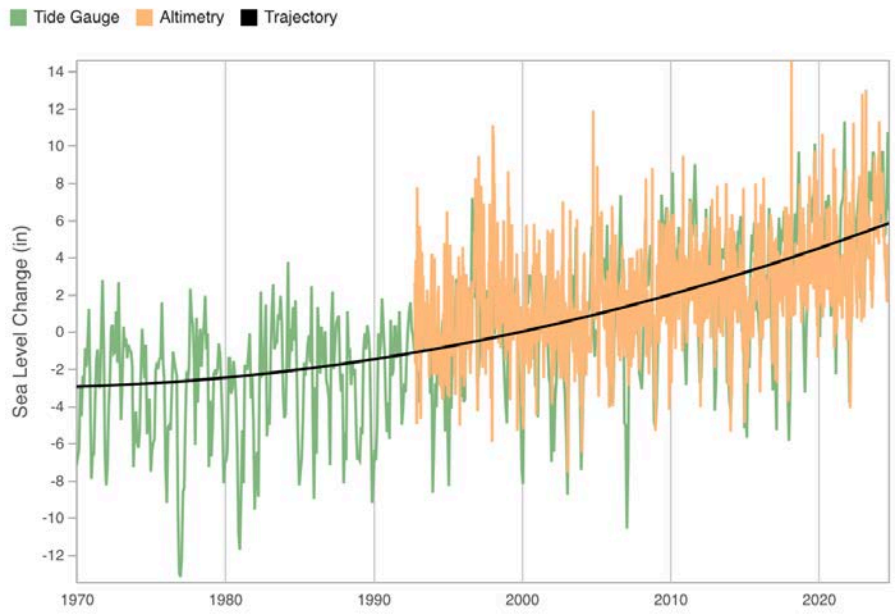

Figure 2. The tide-gauge and satellite altimetry record at The Battery (New York City), NY, showing acceleration from 0.5 inches/decade (1.2 mm/yr) in the 1970s to 2.5 inches/decade (6.3 mm/yr) in the 2010s. Source: https://earth.gov/sealevel/us/national-sea-level-explorer/?psmsl_id=12&scope=section_1

Galveston

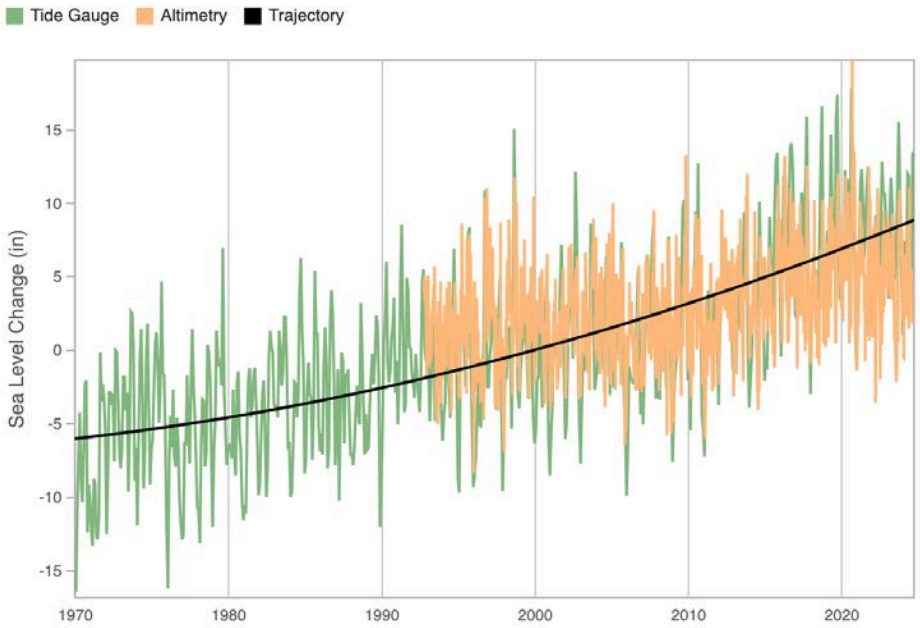

Figure 3. The tide-gauge and satellite altimetry record at Galveston, TX, showing acceleration from 1.4 inches/decade (3.6 mm/yr) in the 1970s to 3.7 inches/decade (9.4 mm/yr) in the 2010s. Source: https://earth.gov/sealevel/us/national-sea-level-explorer/?psmsl_id=161&scope=section_1

Grand Isle

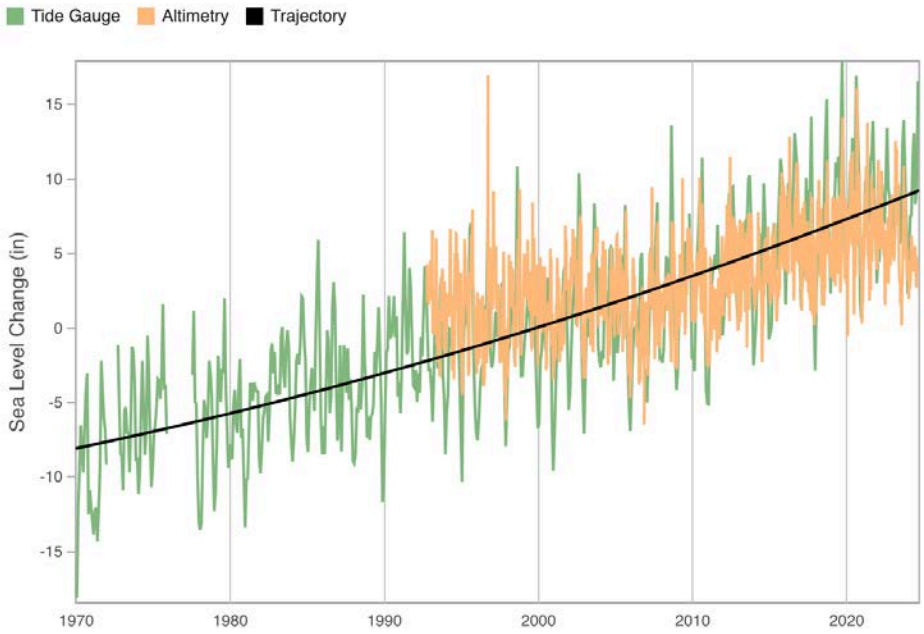

Figure 4. The tide-gauge and satellite altimetry record at Grand Isle, LA showing acceleration from 2.3 inches/decade (5.9 mm/yr) in the 1970s to 3.8 inches/decade (9.6 mm/yr) in the 2010s. Source: https://earth.gov/sealevel/us/national-sea-level-explorer/?psmsl_id=526&scope=section_1

St. Petersburg

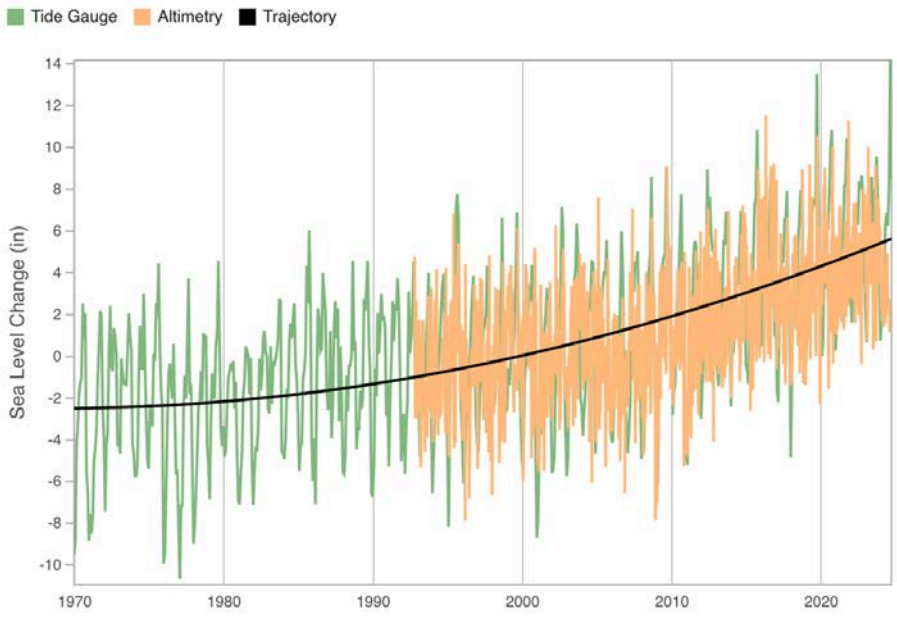

Figure 5. The tide-gauge and satellite altimetry record at St. Petersburg, FL, showing acceleration from 0.3 inches/decade (0.8 mm/yr) in the 1970s to 2.4 inches/decade (6.0 mm/yr) in the 2010s. Source: https://earth.gov/sealevel/us/national-sea-level-explorer/?psmsl_id=520&scope=section_1

San Francisco

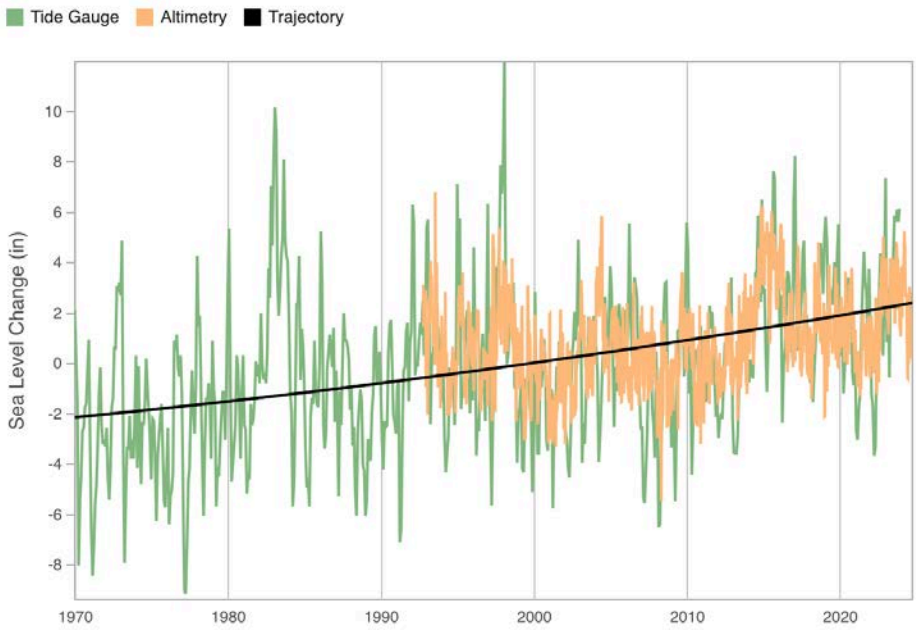

Figure 6. The tide-gauge and satellite altimetry record at San Francisco, CA, showing acceleration from 0.6 inches/decade (1.6 mm/yr) in the 1970s to 1.0 inches/decade (2.5 mm/yr) in the 2010s. Source: https://earth.gov/sealevel/us/national-sea-level-explorer/?psmsl_id=10&scope=section_1

The chapter also fails to acknowledge the substantial scientific literature documenting sea-level acceleration both in the global mean (Cazenave & Moreira, 2022; Dangendorf et al., 2024; Fox-Kemper et al., 2021; Hamlington et al., 2024) (Figure 7) and at a regional scale (e.g., Dangendorf et al., 2023; Ezer, 2023; Kopp, 2013; Walker et al., 2021). As the chapter rightly points out, sea-level rise monitoring over recent decades has been exceptional, providing a valuable understanding that informs future expectations (p. 75). Global-scale monitoring of sea-level rise was first enabled by satellite altimetry, which launched in late 1992 and achieved unprecedented precision in monitoring GMSL change (order of 1 mm yr^{-1}). This precision made possible the detection of a GMSL acceleration of 0.084 ± 0.025 mm yr^{-2} from 1993 to 2018 directly from satellite data (Nerem et al., 2018), the emergence of which was predicted using climate model large ensembles several years earlier (Fasullo et al., 2016). The evident global acceleration of sea-level rise (Figure 7) is not discussed in the report.

The DOE CWG report mentions “current” rates near 0.12 inches/yr (3.0 mm/yr), presumably referring to the 1993-2020 averages (p. 75). In truth, however, GMSL rise has accelerated during that period, and it is now estimated that the GMSL rate has doubled during the three decades of satellite altimetry, increasing from about 0.08 inches/yr (2.1 mm/yr) in 1992 to about 0.18 inches/yr (4.5 mm/yr) in 2024 (Hamlington et al., 2024). The rate of GMSL rise is expected to continue increasing as long as global mean surface temperature continues to increase (Grinsted et al., 2022; e.g., Grinsted & Christensen, 2021; Hermans et al., 2021).

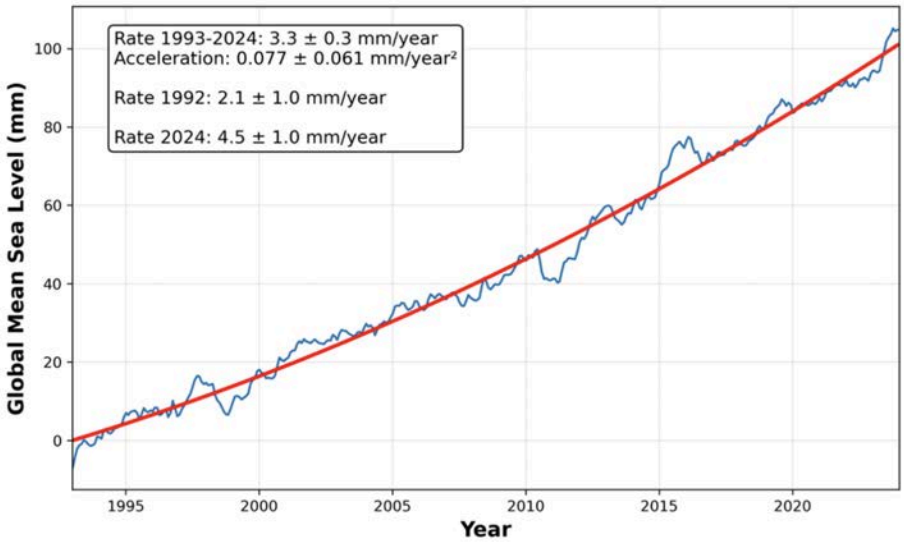

Figure 7. Global mean sea level time series (mm) from satellite altimetry over the time period from 1993 through 2023. The solid red line is the quadratic fit to the data and indicates the sea-level change over the altimeter record relative to 1993. Reproduced from Hamlington et al. (2024).

A missing evaluation of Intergovernmental Panel on Climate Change sea-level projections

¶2 of the chapter summary (p.75) states that "Extreme projections of global sea level rise are associated with an implausible extreme emissions scenario and inclusion of poorly understood processes associated with hypothetical ice sheet instabilities. In evaluating [IPCC] AR6 projections to 2050 (with reference to the baseline period 1995-2014), almost half of the interval has elapsed by 2025, with sea level rising at a lower rate than predicted" (p. 75).

However, the chapter text *does not evaluate* Intergovernmental Panel on Climate Change (IPCC) Sixth Assessment Report (AR6) projections, making it impossible to understand the grounds upon which the authors claim that "sea level [is] rising at a lower rate than predicted." Had the authors conducted such an evaluation, the evaluation would have shown that current rates of sea-level rise are consistent with AR6 projections. Relative to 1995-2014, AR6 estimates a 19-year average global mean sea level centered in 2020 of 4-6 cm (4-7 cm when allowing for rapid ice sheet loss processes). While we do not have the full data set for 2011-2029 to enable calculation of the observed value, extrapolating observations, as in Sweet et al. (2022) or Hamlington et al. (2024), yields a value for 2020 of about 5-6 cm, fully consistent with the AR6 projections.

Similarly, Hamlington et al. (2024) extrapolated the observed GMSL acceleration to 2050, projecting 21 ± 5 cm of GMSL rise between 2008 and 2050. This range is consistent with AR6 projections under all emissions scenarios (Figure 8). Wang et al. (2025) show that observation-based extrapolations of tide-gauge data and AR6 medium-confidence projections agree within the *likely* range for 2050 at 96% of the 222 tide gauges they examine, including The Battery (New York City) tide gauge that the DOE CWG highlights in section 7.3.

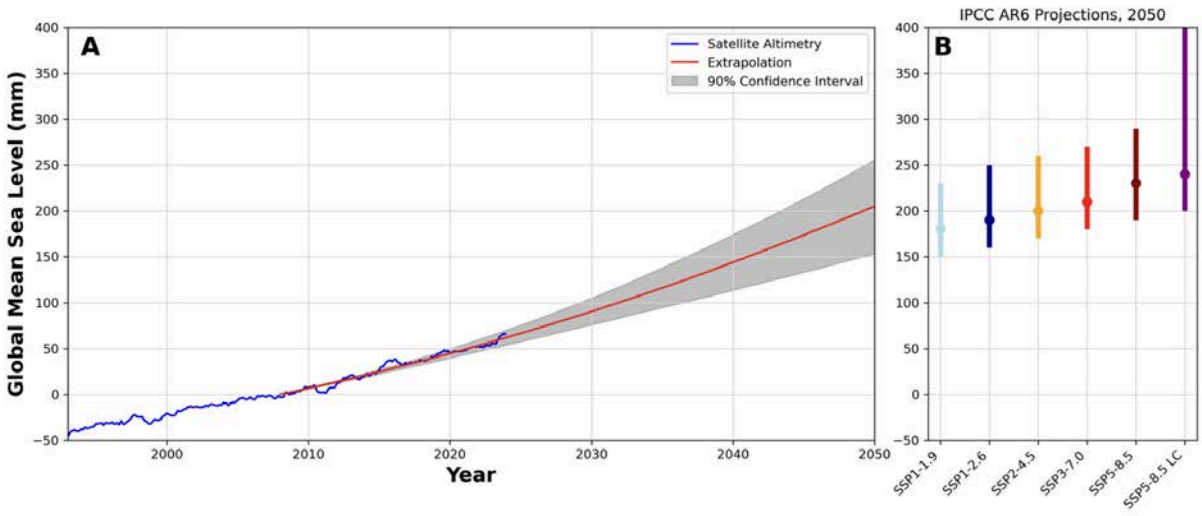

Figure 8. A Extrapolation of the quadratic fit to the satellite altimeter record to 2050. The gray shaded area reflects the 90% confidence interval on the extrapolated sea level. **B** the model-based projections from the IPCC AR6 for six of the different scenarios considered. Error bars on the projections indicate the *likely* range for each scenario. The SSP5-8.5 LC scenario indicates the Low Confidence scenario used in the IPCC AR6. Reproduced from Hamlington et al. (2024).

The chapter text also *does not assess* the extensive and rapidly evolving literature on ice sheet dynamics, mass balance and instabilities (e.g. Millan et al, 2023; Davis et al, 2023; Ryan et al, 2023). Moreover, ice sheet model projections underestimate consensus estimates of ice sheet mass change (e.g., Aschwanden et al., 2021), which reaffirms the potential for sea-level contributions towards the higher end of the AR6 projection range (see also, e.g., Badgeley et al., 2025; Box et al., 2022).

Although the chapter claims to provide an evaluation of sea-level rise projections, Chapter 7 of the DOE CWG also fails to discuss nearly all of the peer-reviewed projection studies of global sea-level rise that have been published in recent years. Between 2013 and 2018 alone, 30 unique, peer-reviewed studies published global sea-level rise projections (Garner et al., 2018). Since 2018, there have been more than a dozen additional peer-reviewed publications producing global sea-level rise projections. Despite the multitude of sea-level rise projections published in the scientific literature, the authors of the DOE CWG discuss only the Sweet et al., 2022, Interagency sea-level scenarios (Sweet et al., 2022), with a cursory mention of the IPCC AR6 projections, from which the Interagency scenarios are derived (Fox-Kemper et al., 2021).

A missing review of impacts of sea-level rise on the United States

Notably absent from chapter 7 (pp. 75-81) of this “critical reviews of impacts” is any review of the impacts of sea-level rise on the United States, either at present or in the future. The closest the chapter gets to addressing this topic is a brief (uncited) dismissal stating that “concern over sea level rise is not about the roughly eight inches of global rise since 1900” (p. 79). In fact, by making coastal flooding more frequent, the effects of historic sea-level rise are already causing measurable economic harm to the United States (e.g., Buchanan et al., 2017; Garner et al.,

2017; Sweet et al., 2024). Other current impacts of historic sea-level rise include more frequent saltwater intrusion into groundwater, more frequent groundwater flooding, and coastal erosion, including loss of tidal wetlands as rates of RSL rise start to exceed the rates at which accretion can keep pace (May et al., 2023; Saintilan et al., 2023).

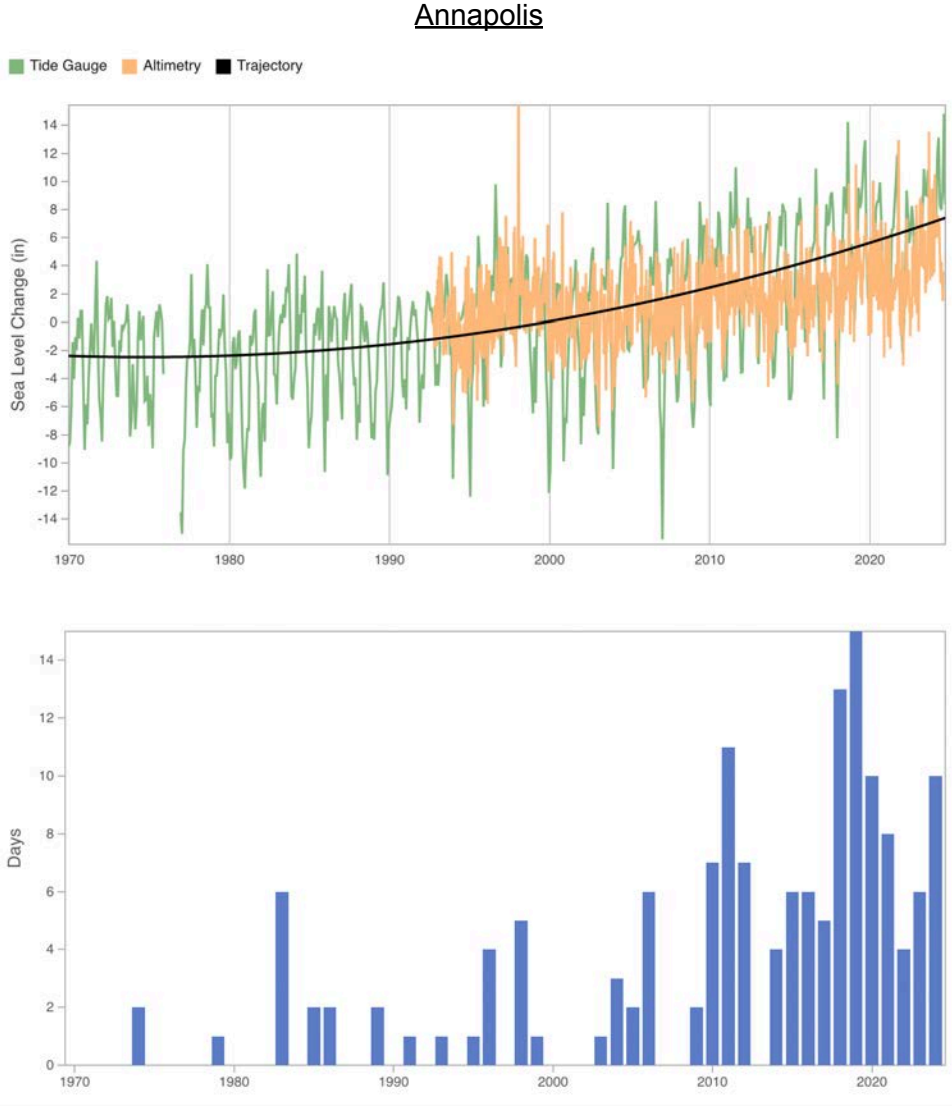

Figure 9. (top) The tide-gauge and satellite altimetry record at Annapolis, MD, showing acceleration from 0.0 inches/decade in the 1970s to 3.1 inches/decade in the 2010s. Source: https://earth.gov/sealevel/us/national-sea-level-explorer/?psmsl_id=360&scope=section_1 (bottom) The number of days per year at Annapolis, MD, at which water levels exceed the NOAA minor flooding threshold. These have grown from 0.3 days/yr on average in the 1970s to 8.3 days/yr on average over 2015–2025, as a result of relative sea-level rise. Source: https://earth.gov/sealevel/us/national-sea-level-explorer/?psmsl_id=311&scope=section_2

Section 7.2, ¶1 (p. 75) makes specific reference to the Chesapeake Bay region, which includes the Nation's capital of Washington, DC, as one area of the U.S. coasts in which sea level increases are associated with local land sinking unrelated to climate change. While the rate of subsidence has remained fairly constant, RSL rise accelerated over 1975 to 2021 at all of the

Bay's tide gauges (Ezer, 2023). While subsidence was just under half of the relative sea-level rise during the mid-20th century, it is now less than 30%.

This RSL rise has driven an escalating frequency of coastal flooding, with measurable economic costs. In 2017, high-tide flooding reduced visits to Annapolis's historic downtown by an estimated 1.7% (95% confidence interval, 1.0-2.6%) (Hino et al., 2019). Annapolis experienced 5 high-tide flooding days in 2017, and an average of 8 per year over the last decade (Figure 9). Even under Sweet et al. (2022)'s Low scenario – which involves a reversal of the current global-mean sea-level rise acceleration, such that the 21st century rate of rise averages 0.12 inches/year (3.0 mm/yr) – Annapolis is expected to experience an average of 75 high tide flood days/year in 2050, compared to 8 per year over the last decade (https://earth.gov/sealevel/us/national-sea-level-explorer/?psmsl_id=311&scope=section_2).

Similar historic increases in coastal flooding are seen elsewhere in the country, and expected relative sea-level rise is projected to increase high-tide flooding by 5-10x on average across the country by 2050 in most locations (May et al., 2023; Sweet et al., 2022).

Tidal flooding delayed commuters in US coastal counties by an average of 23 minutes/year in 2017, compared to about 12 minutes/year in 2002, and that this average delay would increase to 183 minutes/year in 2060 under the Interagency Low scenario (Hauer et al., 2023).

The effects of sea-level rise are also manifest in more extensive, expensive, and frequent storm-driven flooding. For example, about 13% of Hurricane Sandy's damage (about $8 billion) and 9% of its population flood exposure (about 70,000 people) in the New York/New Jersey/Connecticut tri-state area would not have happened without human-caused sea-level rise (Strauss et al., 2021).

Projected sea-level rise will have profound effects on many coastal communities. In the absence of protective measures, sea-level rise under a moderate emissions SSP2-4.5 scenario is projected to displace about 1.5 million Americans by the end of the century, with consequent demographic feedbacks effects leading to a shift in population away from coastal counties of about 15 million people (Hauer et al., 2024). These demographic shifts will significantly affect the economic future of coastal communities, including stresses on local government finances (Gilmore et al., 2022). Increasing coastal flood hazards also risk destabilizing insurance markets (Kousky et al., 2024). In remote regions such as northern Alaska, coastal communities face additional adaptation challenges, in part due to settlement patterns in Indigenous communities which were shaped by colonization (Buzard et al., 2024).

Chapter 7 excludes any analysis of adaptation with respect to sea-level rise. Adaptation can reduce, but not eliminate, damages associated with future sea-level rise. For example, damages to U.S. coastal properties due to anticipated sea-level rise are expected to reach $10–37 billion/year by 2090 in the absence of adaptation, but would still be $7–8 billion/year even with cost-benefit optimal adaptation (Hartin et al., 2023). Yet the United States is not optimally adapted even to current coastal hazards (e.g., Lorie et al., 2020). Economically efficient adaptation will require substantial investments by individuals, industries, and local-to-federal

governments well in advance, and failure to make such investments will decrease economic stability (e.g., Kousky et al., 2024). Furthermore, adaptation planning that neglects diverse communities and histories within the United States, including racialized communities and coastal Tribal Nations, risks exacerbating inequities (Hardy et al., 2017; Leonard, 2021).

In addition, many low-lying regions will reach adaptation limits over the course of this century (Magnan et al., 2022). Expensive wholesale relocation will be required in some areas, which comes with major non-financial costs in terms of employment and family and social cohesion. Planned and managed relocation in response to increased flooding threats affected by sea-level rise has not yet been successful at scale.

Additional comments on 7.1. Global sea level rise (p. 75)

This section is a **technical** comment on section 7.1 of the DOE CWG report (p. 75). It supplements comments made above with respect to treatment of sea-level acceleration in this chapter.

Section 7.1 is an approximately accurate summary of the importance of global-mean sea-level rise, its main drivers, and its historic rates (p. 75).

¶1 (p.75) correctly notes that GMSL rise is driven primarily through thermal expansion, glacier and ice sheet mass loss, and changes in land-water storage–a fact that has been well understood in the scientific community for over four decades (e.g., Gornitz et al., 1982; Hoffman et al., 1983; Revelle, 1983). It also notes that regional sea-level change differs from GMSL change due to changes in (atmosphere/)ocean circulation, vertical land motion, and "geological processes and deformation from the redistribution of ice and water"--again a fact that the scientific community has long understood (e.g., Clark & Lingle, 1977; Gutenberg, 1941). If by "geological processes" the authors intend to refer to the geophysical processes of Earth gravitation and rotation, this statement is conceptually correct though incorrectly articulated. These geophysical processes, together with associated lithospheric and mantle deformation, are referred to as "gravitational, rotational and deformational" (GRD) effects by sea-level scientists (Gregory et al., 2019).

¶2 (p.75) summarizes the AR6 assessment of historic sea-level change. AR6 assesses the very likely range of 1901-2018 GMSL rise to be 150.3-253.5 mm (5.9-10.0 inches), with a median estimate of 201.9 mm (7.9 inches) (Fox-Kemper et al., 2021).[1] Chapter 7's quoted rate of GMSL rise is 0.12 inches/year (3.0 mm/yr), which is slightly lower than the AR6 assessment that the rate of GMSL rise over 1993-2018 was 3.25 (2.88-3.61) mm/yr [0.13 (0.11-0.14) inches/year].

The reference that DOE CWG cites for the rate of GMSL rise does not exist. The text cites NASA. (2020). *NASA sea level rise portal: 2020 edition*. https://www.nasa.gov/specials/sea-level-rise-2020/. The NASA Sea Level Change Portal (sealevel.nasa.gov) never had that title, and the link provided is not to any version of the portal, but rather to a video "Rising Waters," which provides "A look at how NASA is dealing with the

[1] The report incorrectly converts 253.5 mm to 9.8 inches.

threat of sea level rise to its coastal infrastructure, particularly at Langley Research Center in Hampton, Virginia, and Ames Research Center in Mountain View, California."

¶3 (p. 75) states that "Following the end of the Little Ice Age in the mid-nineteenth century, tide gauges show that the global mean sea level began rising during the period 1820-1860, well before most anthropogenic greenhouse gas emissions." It is not possible to evaluate GMSL before the late nineteenth century based on tide gauge data alone, due to the sparsity of records, and the authors provide no citation to support this statement.

Adequately reconstructing GMSL before the 20th century requires the inclusion of geologic proxy data because tide-gauge data are too sparse. For example, by combining tide-gauge and geological data, Walker et al. (Walker et al., 2022) show that "it is very likely that rates of sea-level rise emerged above pre-industrial rates by 1863 CE (P= 0.9; range of 1825 [P= 0.66] to 1873 CE [P= 0.95]), which is similar in timing to evidence for early ocean warming and glacier melt." The partitioning of the drivers of this emergence between natural factors and anthropogenic factors remains unclear. Notably, Santer et al. (Santer et al., 2025) show that an anthropogenic signal in the atmosphere would have been detectable by the 1880s (~290 ppm CO2) (possibly earlier, since their analysis starts in 1850, by which time CO2 concentration had already risen to 285 ppm, above an eighteenth century level of about 277 ppm).

Additional comments on 7.2. U.S. sea level rise (p. 75-79)

This section is a **technical** comment on section 7.2 of the DOE CWG report (p. 75-79). It supplements comments made above with respect to treatment of sea-level acceleration in this chapter.

The purpose of section 7.2 is unclear (p. 75-79). Largely, this section is a tour of 4-5 seemingly arbitrarily selected U.S. tide gauge records, primarily making the point that vertical land motion (of both natural and anthropogenic origin) is an important contributor to relative sea-level rise, and its spatial variability. This statement is true and uncontroversial, as reflected in the NOAA map reproduced as Figure 7.1.

In considering the role of vertical land motion (VLM) in tide-gauge records, it is important to note that subsidence exacerbates the risks of climate-driven sea-level rise. Indeed, if GMSL rise ends up on the lower side of projections, communities with larger subsidence will be more disproportionately at risk. Local subsidence is a risk amplifier, not a reason to ignore the need for adaptation to climate-driven sea-level rise.

By failing to evaluate US tide gauge records in the context of GMSL acceleration, Section 7.2 (p. 75-79) presents an incomplete and skewed perspective.

The presentation in section 7.2 (p. 75-79) stresses the importance of VLM in contrast to rising sea-surface height (what the authors term "absolute sea level rise," or which the modern sea-level research community calls 'geocentric sea-level rise' (Gregory et al., 2019)). It does not consider that the latter is increasingly adding to the threats due to the former. The relative sea-level rise rates provided in Table 7.1 are presumptively century-scale trends, and do not

reflect the acceleration that has been observed in satellite and tide gauge records over the past few decades at many locations (e.g., Dangendorf et al., 2023).

¶2 (p. 76) states that "measurements of relative sea level rise from tide gauges conflate the climatologically relevant increase in the volume of seawater with local vertical land motion." Definitionally, RSL rise is "the change in local mean sea level relative to the local solid surface, i.e., the sea floor" (e.g., Gregory et al., 2019). There are thus two errors in the report statement.

First, measurement of RSL change is not a 'conflation' of a climatic signal; it is a measurement of the climate-impact driver most closely related to the local increasing coastal flood hazard. To say that RSL measurements 'conflate' the increase in seawater volume with vertical land motion suggests a misunderstanding of relative sea-level rise and its importance as a climate-impact driver. Second, the authors' statement seems to imply the only climatologically relevant contributor to RSL change is the change in the global volume of seawater. This implication neglects the role of atmosphere/ocean dynamics, as well as the role of the gravitational and rotational effects of redistributing ice and water mass. Like vertical land motion, these factors "var[y] from place to place."

¶3 and following (p. 76-79), including Figures 7.2-7.5 and Table 7.1. The authors' Table 7.1, and associated text in section 7.2, is of unclear provenance and significance. No sources are provided for the tide gauges or VLM data. No confidence intervals are quoted in this table, nor are the methods used to calculate the rates anywhere documented. Though the records discussed are all of different lengths, the time period(s) over which rates are quoted are not specified.

We expect the time period over which VLM is estimated is different than the time period over which RSL rise is estimated (since GPS data, the likely source, has been available only in recent decades); whether VLM rates can be assumed constant and therefore can appropriately be extrapolated for the length of the tide gauge record will differ depending on the driver. Constant rates are a fine assumption if glacial-isostatic adjustment is the dominant driver, as in New York, but not if anthropogenic factors dominate, as in Galveston and Grand Isle (Kearns et al., 2015; Kolker et al., 2011).

Additional comments on 7.3. Projected sea level rise (p. 79-80)

This section is a **technical** comment on section 7.3 of the DOE CWG report (p. 79-80). It supplements the comments above on evaluation of sea-level projections and on the impacts of sea-level rise.

Section 7.3 (p. 79-80) is largely a summary of the IPCC AR6 assessment of projected sea-level rise, with the final paragraph critiquing the US Interagency sea-level rise scenarios, as misinterpreted by the authors.

¶2-¶3 (p. 79-80). These paragraphs attempt to summarize IPCC AR6 sea-level rise projections. (The projections, both globally and locally, are readily available on the web at https://sealevel.nasa.gov/ipcc-ar6-sea-level-projection-tool.)

The numbers quoted for AR6 by the DOE CWG are difficult to trace, because the authors do not appear to understand the role of assessment language in the IPCC. A “likely” or “very likely” assessment is not simply a mechanical reporting of a computed probability distribution, but an expert assessment that there is at least a 66% (likely) or 90% (very likely) chance the correct value falls within the quoted range. AR6 deliberately *did not* assess *very likely* ranges for projected sea-level rise, given the potential contributions from deeply uncertain processes; instead it assessed the *likely* contributions (only) from those processes characterized by at least medium confidence. 5th--95th percentile ranges are computed and reported in the underlying data sets, but are *not* assessed ‘very likely’ ranges. Considering only processes with at least medium confidence, across all emissions scenarios, AR6 assessed a *likely* rise in 2050 between 15–29 cm (6–11 inches), relative to a 1995-2014 baseline. For 2100 in SSP2-4.5, the corresponding *likely range* is 44–76 cm (17–30 inches). See Kopp et al. (Kopp et al., 2023) for a discussion regarding the communication of uncertainty in IPCC sea level projections.

(Also note that at a few places in Chapter 7 of the DOE CWG, numbers reported to centimeter precision are reported to the nearest hundredth of an inch (i.e., nearest 0.03 cm), which vastly overstates the statistical confidence. This practice is inappropriate (Taylor, 1997) and characteristic of a general disregard for appropriate use of statistics throughout the chapter.)

¶4 (p. 80) and Figure 7.6 purport to compare rates of RSL rise at the Battery to the “NOAA” (US Interagency) sea-level rise scenarios (Sweet et al., 2022), arguing that, “[Sweet et al., 2022] claim that by 2050, the sea will have risen one foot at The Battery in Manhattan (relative to 2020)”. There are several problems with this comparison, which misconstrues the purpose of the Interagency planning scenarios, cherry picks a single scenario in a manner that neglects the range of estimates associated with it, and inaccurately reports scenario sea levels. (The following analysis uses the data sets available for download at https://sealevel.nasa.gov/ipcc-ar6-sea-level-projection-tool and https://sealevel.nasa.gov/task-force-scenario-tool).

The Interagency report provides a set of scenarios to support decision making, derived from the science in AR6. These scenarios are based on assumed amounts of sea-level rise by 2100, and they are intended to support different decision contexts by spanning a broad range of possible future outcomes. The Interagency scenarios are not independent of the AR6 projections, the evaluation of which we discussed above.

The Interagency report includes five scenarios. The “Low” scenario corresponds to a reversion of rates of GMSL rise to their early 21st century average, such that the average 21st century rate would be 3.0 mm/yr. The “Intermediate-Low” scenario is consistent with the AR6 median projection under a sub-2°C warming scenario (SSP1-2.6), and the “Intermediate” scenario is consistent with the upper end of likely range projections in the absence of deeply uncertain rapid ice sheet loss processes. The “Intermediate-High” and “High” scenarios require increasingly significant contributions from such ice sheet processes.

At the Battery, the Interagency scenarios reflect (Figure 10):

- Low: 21 cm of increase from 2020-2050 (range of 15-27 cm)
- Intermediate-Low: 24 cm of increase from 2020-2050 (range of 19-30 cm)
- Intermediate: 27 cm of increase from 2020-2050 (range of 22-34 cm)
- Intermediate-High: 33 cm of increase from 2020-2050 (range of 25-44 cm)
- High: 37 cm of increase from 2020-2050 (range of 27-50 cm)

It is thus unclear where the authors would have gotten a “one-foot [i.e., 30 cm] rise in thirty years.

Again, these are scenarios for planning; to assess likelihoods, we must turn to AR6. At the Battery, AR6 (medium confidence) assesses a *likely* RSL rise in 2050 (relative to 2005) of 24-50 cm, across emissions scenarios. Under SSP2-4.5, the median projection is for 26 cm of RSL rise between the 19-year period centered on 2020 and the 19-year period centered on 2050 (thus, an average rate of about 3.4 inches/decade). This is consistent with the Interagency report’s extrapolation of the observed acceleration since 1970 (Figure 10).

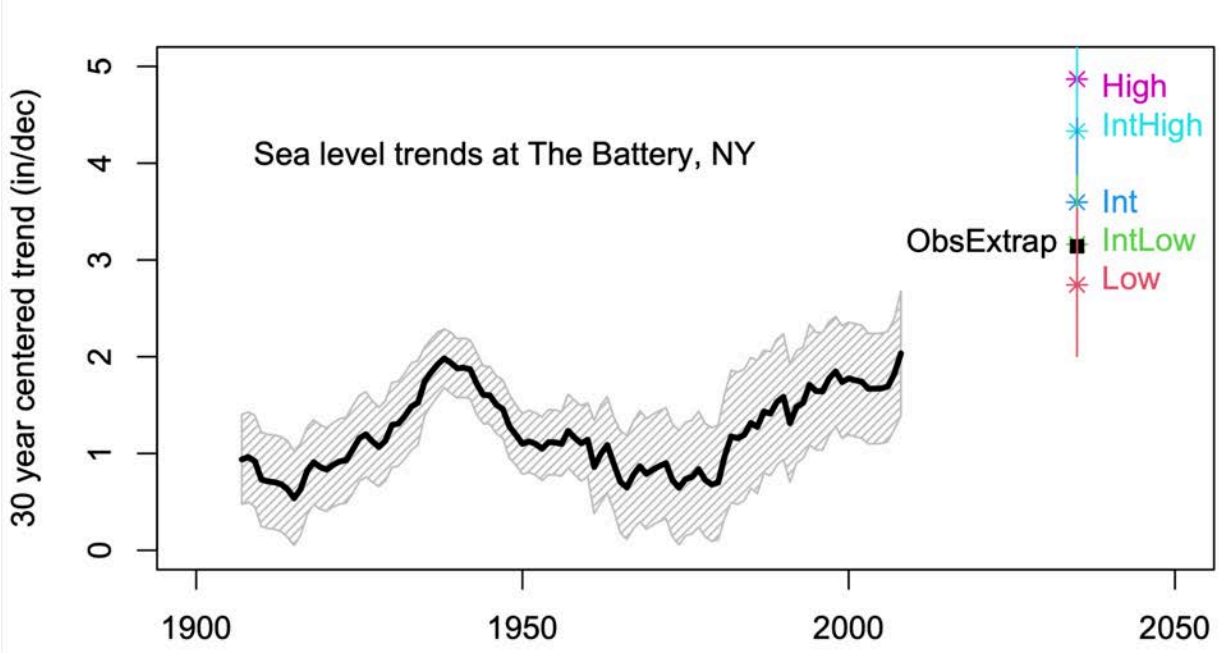

Figure 10. 30-year centered trends in the Battery tide gauge record, compared to the 2020-2050 average rates of increase in the 5 Interagency scenarios (Sweet et al., 2022) and the extrapolation of observations since 1970. (See Figure 2 for the trajectory underlying the extrapolation.)

¶4 (p. 80) and Figure 7.6 also reflect a more fundamental issue: the implicit message of this paragraph is that projections should be evaluated one tide gauge at a time. This comparison might make sense if tide gauges reflected independent time series, and there was no understanding of the physical processes that – individually and collectively – drove the changes observed at tide gauges. This situation is not, however, the case.

A comparison of tide gauge data to projections one tide gauge at a time is inherently flawed. Data-model comparisons must account for the totality of evidence — the global set of tide

gauge data, satellite observations of changes in sea surface height, observations of changes in ocean heat context and land ice mass, the scaling relationship between global-mean surface temperature and rate of sea-level rise, etc. More local and variable factors, such as atmosphere-ocean dynamics, can lead to misleading inferences if the global data set and context is excluded, as in the report's analysis.

The relatively high rate of sea-level rise at the Battery in the 1930s, for example, occurred in association with a relatively rapid increase in Northern Hemisphere extratropical temperature (about 0.2°C/decade warming from 1910-1940; compare 0.07°C/decade *cooling* from 1940-1980 and 0.4°C/decade warming from 1980-2020; 24°N-90°N averages from GISTEMP Team, 2025), an associated reduction in glacier ice mass, and an associated period of relatively high rates of GMSL rise (about 2 mm/yr in the late 1930s, vs. about 1 mm/yr in 1960s and more than 4 mm/yr today) (Dangendorf et al., 2024).

Comprehensive analyses are more challenging than simply plotting projections at one tide gauge against that tide gauge's data, but exist in literature not cited by the DOE CWG report. Dangendorf et al. (2024), for example, present a reconstruction of sea level over space and time that is informed by the global set of tide gauge data as well as a variety of constraints on land ice and water storage changes. They find clear evidence of a sustained acceleration in rates of GMSL change since the 1970, as well as in sea-level rise in the subtropical North Atlantic (which includes the US Atlantic and Gulf Coasts), the South Atlantic, and the East Pacific. As noted above, the observed acceleration is consistent with Interagency scenarios.

In considering projections of future sea-level rise, it is important to recognize that rates of sea-level rise are increasing *because human activities are changing the climate*. Given rising temperatures, it would be surprising and contrary to current understanding of the physical system for the rate of GMSL rise not to accelerate (Grinsted & Christensen, 2021). With global mean surface temperature continuing to rise, we should indeed expect future rates of global sea-level rise – which, in the twentieth century, were already the highest in at least 3000 years (Kemp et al., 2018; Kopp et al., 2016; Walker et al., 2021) – to be "remarkable" in "historical context" (p. 80).

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Comment on Section 8, Uncertainties in Climate Change Attribution

Nathan Gillett, CCCma, Environment and Climate Change Canada

Page: 84

Comment type: Technical

Comment: The paragraph on solar variability quotes an estimate of radiative forcing due to TSI changes over the period 1750-2011 from AR5 (0.05 W /m^2). It then quotes a change in TSI between the Maunder Minimum and the second half of the twentieth century of 0.7-2.7 W/m^2 from Gulev et al. (2021). But the associated radiative forcing change is much less than the TSI change (because it is an average over the surface area of the globe, and because some irradiance is reflected). Gulev et al. (2021) follow the quoted TSI change with the sentence “This TSI increase implies a change in ERF of 0.09–0.35 W m^{-2} “. The authors should quote this ERF change not the TSI change for comparability with the ERF change quoted earlier in the paragraph.

Page: 85

Comment type: Technical

Comment: Kravtsov et al. (2024) analysed the CMIP5 climate models. However, the more recent CMIP6 climate models as a group show more multi-decadal variability than the CMIP5 models. Eyring et al. (2021) assessed “Parsons et al. (2020) found that some CMIP6 models exhibit much higher multi-decadal variability in GSAT than CMIP5 models, with indications that variability in these models is also higher than that from proxy reconstructions”. The authors should cite and discuss Parsons et al. (2020) and the multi-decadal variability in CMIP6 models here.

Page: 85

Comment type: Technical

Comment: The authors write “However, the recommended forcing dataset for the CMIP6 climate model simulations used in AR6 for attribution studies averages two data sets with low solar variability (Matthes, 2017). While AR6 shows a substantially greater solar impact than does AR5, the overall impact of solar forcing on the climate was still assessed to be small compared to anthropogenic forcing. However, the impact of solar variations on the climate is uncertain and subject to substantial debate (Lockwood, 2012; Connolly *et al.*, 2021) - something that is not evident in the IPCC assessment reports. ” In the executive summary, they write “Moreover, solar activity's contribution to the late 20th century warming might be underestimated [Section 8.3.1].”.

But, as the authors themselves explain, many attribution studies use regression to estimate the contribution of individual forcings to observed temperature changes. Two of the studies used to

estimate anthropogenic and natural contributions to observed temperature change by Eyring et al. (2021) in the latest IPCC report, constrain the amplitude of the naturally-induced temperature changes directly from observed temperature changes in a regression (Haustein et al., 2017; Gillett et al., 2021). If the simulated response to natural forcings were underestimated in the models, this would be corrected in the regression – the simulated response would have to be scaled up to fit observations. There is no evidence of this (Eyring et al., 2021). The authors should revise the text to reflect this.

Pg: 86

Comment type: Technical

Comment: The authors write “Once the simulations of the ensemble members and different models are averaged, the natural internal variability contribution is averaged out owing to differences in phasing, effectively cancelling the role of natural internal variability in the attribution process.” This is incorrect. While ensemble means are indeed used to estimate the forced response from climate model simulations in standard optimal fingerprinting, internal variability is estimated directly from observations, as a residual from the regression against simulated forced responses. This is compared with simulated variability using a residual test to examine whether or not the residual variability is consistent with simulated internal variability. See for example Allen and Tett (1999), or Ribes et al. (2013b) for discussion of the residual test. The authors should correct this error.

Pg 88:

Comment type: Technical

The authors write “While [optimal fingerprinting] has been widely used and prominently featured by the IPCC since 2001, there is very little literature examining the statistical properties of the results it generates.” This is not the case. There is extensive literature on the statistics of optimal fingerprinting. See for example the review in Eyring et al. (2021), Section 3.2.1. Or see Ribes et al. (2013a; 2013b). The authors should correct this error.

Pg 89:

Comment type: Technical

Comment: The authors write “Under conditions that easily arise in optimal fingerprinting, TLS estimates have a large positive bias. Thus, any study that used TLS for optimal fingerprinting without verifying its applicability in the specific data context has likely overstated the attribution.” IPCC (2021) based its assessment of human-induced climate change primarily on the results of three approaches: Haustein et al. (2017); Ribes et al. (2021) and Gillett et al. (2021), as described in Eyring et al. (2021). Of these three studies, the only one which used TLS regression was Gillett et al. (2021).

Gillett et al. (2021) did indeed verify the applicability of the TLS results by applying an imperfect model test to evaluate the approach – a historical simulation from one model was treated as pseudo-observations, and the TLS approach with simulations from the remaining models was used to estimate the response to aerosols, greenhouse gases and other anthropogenic forcings,

and natural forcings in the withheld model. This was then compared with that simulated directly by the model. No bias in the results was found – see Gillett et al. (2021), Figure 3. The authors should describe how Gillett et al. (2021) find no such large positive bias in a practical implementation of TLS.

Pg 89:

Comment type: Technical

Comment: The authors write “In summary, the primary statistical methods for attributing causation in climate data are optimal fingerprinting and time series analysis. Applications of the Allen and Tett (1999) optimal fingerprinting method dominate the attribution literature and have underpinned past IPCC conclusions, but results depend on the accuracy of climate models and the method has recently been criticized as inherently biased.” IPCC (2021) based its quantitative assessment of human-induced climate change on the results three approaches: Haustein et al. (2017); Ribes et al. (2021) and Gillett et al. (2021), as described in Eyring et al. (2021). Haustein et al. (2017) used a simple Ordinary Least Squares regression, and output from an energy balance model. Ribes et al. (2021), did not use a regression at all, but rather a kriging approach. Gillett et al. (2021) used a total least squares regression. All three studies gave similar results (see Eyring et al. (2021), Figure 3.8). Moreover, a bottom-up approach which did not use optimal fingerprinting or climate models at all (as described in IPCC AR6 WGI Chapter 7, and as also shown in Eyring et al. (2021), Figure 3.8) also gave similar results. Therefore the attribution results described in the most recent IPCC report are not based on a single approach – rather they are based on multiple consistent lines of evidence. The authors should revise this text to reflect this.

Pg 97:

Comment type: Technical

Comment: The authors write “peer-reviewed analyses showed that the event [the 2021 PNW heat wave] was caused by rare meteorological conditions that were not made more probable by global warming.” This statement is misleading. We know that extreme heat events are always a combination of weather variability and climate change. The statement the authors are making is that the weather variability component is not affected by climate change. That may be correct. However, this weather variability is occurring on top of a warmer climate, which makes the event more severe. In fact, the literature they cite themselves concludes that this event was made hotter by human-induced climate change. The authors should revise this statement to say that human-induced climate change made the event hotter.

Page: ix, Executive Summary

Comment type: Technical

Comment: The authors write “Global climate models generally run “hot” in their description of the climate of the past few decades − too much warming at the surface”. This is not correct. Eyring et al. (2021) assessed “The CMIP6 multi-model mean GSAT warming between 1850–1900 and 2010–2019 and associated 5–95% range is 1.09 [0.66 to 1.64] °C. Cross-Chapter Box 2.3 assessed GSAT warming over the same period at 1.06 [0.88 to 1.21] °C.

So some CMIP6 models simulate a warming that is smaller than the assessed observed range, and other CMIP6 models simulate a warming that is larger." The executive summary should be revised to reflect that some global climate models warm less than observations, and that on average the CMIP6 models match observed warming well.

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Response to Section 8.2: Attribution methods

By Christopher Callahan, Ph.D., Indiana University

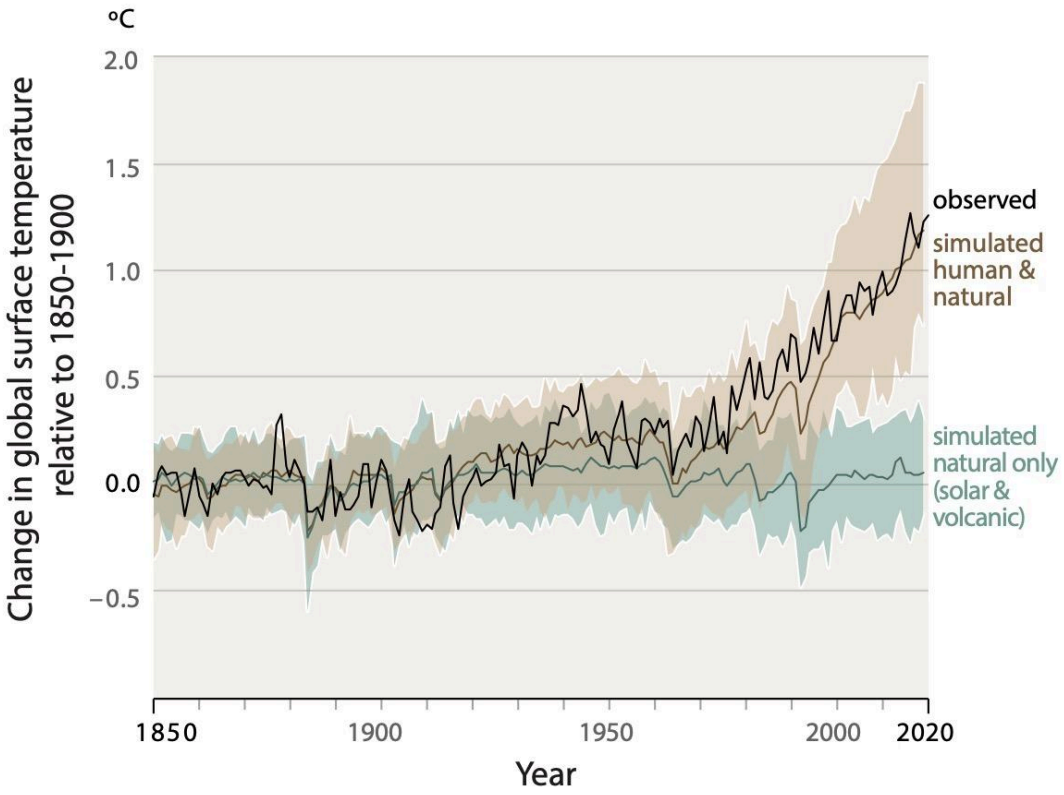
The section on **page 83** of the DOE CWG report outlines their perspective on methods used to attribute observed changes in the climate to human influence.

Comment: Editorial

The DOE CWG report outlines “optimal fingerprinting,” “time series analysis,” and “process-based attribution” as methods for attributing climate changes to anthropogenic influence (extreme event attribution will be left to a separate discussion). While each method is indeed used, the DOE CWG report neglects more simple and fundamental ways of understanding the human influence on observed climate change.

DOE neglects theory and early models. As early as the 1850s, Eunice Foote (1856) and John Tyndall (1861) developed the early science of the greenhouse effect. In 1967, Syukuro Manabe and Richard Wetherald developed the first iterations of computational climate models in radiative-convective equilibrium (Manabe & Wetherald, 1967). These models and the theory underpinning them led to the prediction, among others, that greenhouse gas forcing should warm the troposphere and cool the stratosphere. Observed patterns of tropospheric warming and stratospheric cooling confirm these predictions and cannot be comprehensively explained by mechanisms other than greenhouse gas forcing (Santer et al., 1996). A similar prediction has been borne out for extreme precipitation; the Clausius-Clapeyron relationship predicts that warmer air holds 7% more water vapor per K, and the observed increase in extreme rainfall globally has supported this prediction (Fischer & Knutti, 2016).

Coordinated climate model comparison enables attribution without specialized statistical techniques. Over several iterations, the Coupled Model Intercomparison Project has coordinated climate model experiments where models are run with all historically observed forcings, and with natural forcings only. It is straightforward to simply compare the time series of observed global temperature with these two groups of forcings. Figure SPM.1 in the IPCC’s Sixth Assessment Report (WGI) shows exactly such a comparison, reproduced below (IPCC, 2021). It is abundantly clear that natural forcings alone cannot explain the historical increase in global temperature.



Adapted by the author from panel (b) of Figure SPM.1, IPCC (2021). "Changes in global surface temperature over the past 170 years (black line) relative to 1850–1900 and annually averaged, compared to Coupled Model Intercomparison Project Phase 6 (CMIP6) climate model simulations…of the temperature response to both human and natural drivers (brown) and to only natural drivers (solar and volcanic activity, green). Solid coloured lines show the multi-model average, and coloured shades show the very likely range of simulations."

DOE neglects consilience across multiple lines of evidence. The IPCC has used multiple lines of evidence to arrive at the conclusion that human influence on the climate system is "unequivocal." Fundamental theory and simple energy-balance calculations yield consistent estimates of human-caused radiative forcing, and changes in global sea level, the water cycle, the cryosphere, and other domains are only consistent with a human influence on the climate system (D. Chen et al., 2021). The strongest case for attribution is that there is *consilience* across many lines of evidence, instead of reliance on one method. These other lines of evidence are not discussed by the DOE CWG report. In the words of Trenberth (2011): "Humans are changing our climate. There is no doubt whatsoever."

In order to effectively synthesize methods used for attribution, the DOE CWG report would need to discuss the theoretical basis for attribution and the role of coordinated climate model intercomparison, and it would need to outline how their selected types of attribution methods fit into the multiple-lines-of-evidence approach taken by the IPCC.

Response to Section 8.3.2: Optimal fingerprinting
By Christopher Callahan, Ph.D., Indiana University

This section addresses **pages 88 and 89** of the DOE CWG report. In this section, the DOE CWG discusses the "optimal fingerprinting" method of climate attribution, which uses linear regression techniques to establish whether observed warming patterns are consistent with anthropogenic or natural forcings. Typically, theory or climate models are used to establish a searched-for "fingerprint" that is characteristic of a particular type of climate forcing, such as the pattern of tropospheric warming and stratospheric cooling yielded by greenhouse gas forcing. The searched-for fingerprint is then convolved with climate observations to assess whether the observations exhibit the fingerprint above some level that might be explained by natural climate variability.

Comment: Reference

Pages 88 and 89 of the DOE CWG report cite four papers—all single-authored by one member of the CWG—to argue that some optimal fingerprinting approaches may not meet the Gauss-Markov conditions required for linear regression to be the best linear unbiased estimator. This line of criticism is primarily focused on the work of Allen and Tett (1999).

This section of the DOE CWG report presents a **misleading sample of the wide array of evidence on fingerprint-based attribution**. The CWG report cites four papers by one of the authors of the report, a reply by Chen et al. (2024), and the original Allen and Tett (1999) paper. They neglect (1) more recent developments in optimal fingerprinting; and (2) non-regression-based fingerprint attribution approaches.

(1) Optimal fingerprinting did not end with Allen and Tett's work. Significant further developments have advanced this field that are relevant to—but not cited by—the DOE CWG report. For example, a main critique leveled by McKitrick (2021) at the original work of Allen and Tett (1999) pertains to conditional invariance—i.e., that the expected value of the residuals conditional on the regressors is 0 ($E[u|X]=0$). One physical interpretation of this statement is that optimal fingerprinting assumes that anthropogenic forcing does not project onto internal variability; attribution studies must ensure that some component of the anthropogenic signal is not aliased into their estimate of natural variability (and vice versa). This issue has been the subject of extensive research. For example, Sippel et al. (2021) use a statistical learning technique to estimate anthropogenic fingerprints that explicitly protect against internal variability contaminating the forced signal. Recent work has also shown that the detection of human influence on global warming is not highly sensitive to how internal variability is characterized (Imbers et al., 2014).

(2) A related but distinct approach to attribution uses pattern similarity methods to determine whether observations are growing more similar to the vertical or spatial pattern of temperature change characteristic of global warming (Ramaswamy et al., 2006; Santer

et al., 1996, 2003, 2018). Typically, the statistic of interest in these approaches is a simple centered or uncentered pattern correlation statistic (Santer et al., 1996). Several types of climate model simulations can additionally be used to determine whether the pattern similarity between observations and the fingerprint is statistically distinguishable from natural variability. Recently, it was shown that the degree of pattern similarity in observations is more than 5 standard deviations ("5 sigma") greater than the range of natural variability, a degree of confidence equivalent to the gold standard in particle physics (Santer et al., 2019). While conceptually similar to the specific TLS approach critiqued by the DOE CWG report, this approach does not use the regression specification of Allen and Tett (1999), nor does it require estimating a regression coefficient and assessing whether it is consistent with unity.

Overall, the report does not treat optimal fingerprinting as a field worthy of surveying and evaluating. Instead, they cite a single paper from 25 years ago, four papers from one author of the DOE report, and no further evidence. To effectively review this literature, the DOE CWG report would need to evaluate how optimal fingerprinting has matured as a method, including the use of tools such as regularization and statistical learning, and place it in the context of other approaches to fingerprint-based attribution.

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Natural variability of large-scale ocean circulations

Graeme MacGilchrist[1], Kim M. Cobb[2]

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This is a **technical comment** on the second subsection of Section 8.3.1 titled “Natural variability of large-scale ocean circulations (pp. 85-88).

In this subsection, the DOE CWG report discusses multi-decadal variability in the coupled atmosphere-ocean system and its impact on the overall trend in global surface temperature. The authors first imply that natural modes of variability in the ocean-atmosphere system are not properly accounted for in the attribution methods of integrated assessment reports such as the IPCC’s AR5 and AR6. They go on to discuss distinct periods over the historical record when natural variability may have impacted observed temperature trends.

Ultimately, this subsection fails to substantiate the claim at the top of Section 8.3 that the latest assessments of the role of internal variability in global warming are “inadequate”. Along the way, it commits several errors of interpretation and understanding, incomplete assessment of the scientific literature, and possible misquoting of IPCC reports.

To address concerns outlined in more detail below, the authors will need to:

1. Provide more relevant information on the assessment of internal variability in the scientific literature and IPCC reports and the methods used, and explain the specific issues that lead the authors to question its adequacy. In particular, the authors must show why they believe that the IPCC-assessed contribution of internal variability to historical warming (±0.2°C) needs to be increased.
2. Update their assertions about the representation of decadal timescale variability in climate models to include a wider swathe of the recent literature, as well as acknowledging the challenges in assessing this due to the short observational record and the key role for paleoclimate data and modeling in assessments of internal vs forced global temperature variability in recent centuries.
3. Include a discussion of the possible, and as yet undetermined, role of forcing in aliasing multi-decadal variability, particularly for Atlantic Multidecadal Variability.
4. Revise incorrect statements about (a) methodologies used to assess internal variability (e.g. relating to model averaging) and (b) the IPCC internal variability range (i.e. its erroneous interpretation as “peak-to-trough”).
5. Provide direct (chapter and subsection) citations for the quotes attributed to the IPCC AR5 and AR6. If these are not forthcoming, the quotations should be removed and

replaced with statements that accurately reflect the IPCC's assessment of, and confidence in, the impact of internal variability on the global warming trend.

6. Either (a) remove the discussion of individual time periods in the latter part of the subsection, which fails to address the role of internal variability in the long term trend, or (b) clearly acknowledge that these statements do not challenge the IPCC assessment of internal variability or the overall attribution of multi-decadal warming to human-induced causes, and considerably expand the discussion to include the instances in which the suspected role of internal variability was to suppress GHG-induced warming.

General comments

Mischaracterization of scientific literature on the impact of internal variability. That decadal-timescale coupled atmosphere-ocean variability impacts global mean temperatures is entirely uncontroversial and widely studied. The topic receives an abundance of attention in the scientific literature, including in both AR5 and AR6 (Bindoff et al., 2013; Eyring et al., 2021). It is a topic of ongoing debate and discussion (Mann et al., 2020; Klavans et al., 2025; Studholme et al., 2025; Barghorn et al., 2025), however the DOE CWG report's suggestion that the contribution of ocean circulation variability to the overall trend in surface temperatures has been missed, or otherwise under-represented in prior climate assessments, is demonstrably false. To meaningfully support their claim that the IPCC-assessed contribution of internal variability to historical warming (±0.2°C) is "inadequate", they need to provide a quantitative and comprehensive assessment of evidence that it should be higher.

Incorrect understanding of how internal variability is assessed in models and in integrated assessments. The DOE CWG report betrays a lack of understanding of how the impact of internal variability is evaluated in attribution studies and the IPCC reports. They note that (pp. 85-86), because in any given simulation the phasing of internal variability will be distinct, averaging across simulations removes the contribution of this variability. Indeed, this is the purpose of averaging simulations (as well as minimizing the effect of model structural uncertainty). No attribution study considers the evolution of the multi-model mean as providing a means of assessing internal variability. Rather, deviations of the observed time-series of warming from the multi-model mean often indicate the imprint of internal variability, as in the case of suppressed warming in the early 21st century (Kosaka and Xie, 2013) and over the satellite era (Po-Chedley et al., 2021, 2022). The contribution of internal variability is instead estimated from various factors including unforced model variability and its inter-model spread (Gillett et al., 2016; Parsons et al., 2020), large ensemble simulations (Christiansen et al., 2025), statistical assessment of the observational record (McKinnon and Deser, 2018), and proxy-derived temperature reconstructions (PAGES2k Consortium, 2019). It is a combination of these assessments that leads the IPCC AR6 to conclude their likely range of ±0.2°C.

Irrelevant focus on specific periods of warming and cooling in the historical record. In the latter part of the subsection (pp. 86-88) the authors examine drivers of individual decadal or multi-decadal periods of warming and cooling. Their assessment focuses on specific time periods but does nothing to address or reconsider the attribution of the multi-decadal warming

trend, which is the purported aim of Chapter 8.3 (p. 84). It is the long term trend that is the salient feature of anthropogenic climate change and which is the focus of attribution studies and IPCC assessment reports. This human-induced warming is widely acknowledged to be overlain by periods of additional warming or cooling associated with internal natural variability in the coupled ocean-atmosphere system. As such, the author's statements are in no way controversial, and ultimately do not support their assertion at the top of Chapter 8.3 that there has been "inadequate assessment of natural climate variability".

Incomplete discussion of coupled model performance. The assertion (p. 85) that "most models have too little amplitude compared to observations at multi-decadal frequencies" is an over-simplification of the latest scientific understanding. In the first instance, assessing multi-decadal variability is challenging due to the relatively short observational record (Cassou et al., 2018) and the fact that these observations exist in a time of forced change (Brown et al., 2017). Nevertheless, comprehensive recent assessments of model performance with respect to ENSO and the PDO suggest that models actually exhibit low frequency variability of these modes (decadal and longer timescale) matching or exceeding observed variability (Fasullo et al., 2020; Parsons et al., 2020). Assessment of longer timescale modes, such as Atlantic Multidecadal Variability, is more challenging with the short observational record (Fasullo et al., 2020) but comparison to proxy-derived temperature reconstructions over the pre-industrial era indicate that many simulations exhibit comparable or greater variability on these timescales (PAGES2k Consortium, 2019). The DOE CWG report also neglects to highlight that the evaluation of model adequacy for the assessment of internal variability focuses not only on the period and amplitude of variability, but also its spatial pattern (Fasullo et al., 2020; Parsons et al., 2020; Eyring et al., 2021). Since the pattern of temperature change associated with GHG-forced global warming is uniquely distinct from that of the leading modes of internal variability, the pattern of overall trends can be used to detect and assess the contribution from each.

Absent discussion of the imprint of forcing in coupled ocean-atmosphere modes. Due to the relatively short observational period and the presence of forced change over this time, there is substantial debate about whether the Atlantic Multidecadal Oscillation represents a mode of internal ocean-atmosphere variability or if it reflects a response to trends in the forcing (Mann et al., 2020; Barghorn et al., 2025). Although debate continues, it is considered plausible or even likely that a substantial component of the Atlantic variability on multi-decadal timescales is driven by changes in regional forcing — namely aerosol loading and GHG-derived radiative forcing — which has been particularly pronounced over the North Atlantic over the last ~70 years decades. The DOE CWG report fails to discuss this central focus in recent literature, despite its evident relevance to the question of attribution.

Potential misattribution of IPCC summary statements. (p. 86) The DOE CWG produces two quotes that it claims to be summary statements from AR5 and AR6 from the IPCC. The quotes suggest that the IPCC reports conclude that the role of forced and natural variability are "difficult to disentangle" and thus that natural variability "contribute[s] to uncertainties in attributing observed changes to anthropogenic forcing". The citations for these quotes are not specific,

referencing the entire IPCC Working Group 1 reports. An exhaustive search in both AR5 WG1 and AR6 WG1 did not find these statements, or anything resembling them. They need to provide exact citations for these quotes. If they are not direct quotes, that needs to be fixed.

IPCC statements that *are* relevant to the impact and representation of internal variability include:

> "Observed warming over the past 60 years is far outside the range of internal climate variability estimated from pre-instrumental data, and it is also far outside the range of internal variability simulated in climate models. Model-based simulations of internal variability are assessed to be adequate to make this assessment. Further, the spatial pattern of observed warming differs from those associated with internal variability. Based on this evidence, the contribution of internal variability to the 1951–2010 GMST trend was assessed to be likely between –0.1°C and 0.1°C, and it is virtually certain that warming since 1951 cannot be explained by internal variability alone." (Stocker et al., 2013)

> "Since AR5, the increased use of 'large ensembles' [...] supports improved understanding of the relative roles of internal variability and forced change in the climate system. Simulations and understanding of modes of climate variability, including teleconnections, have improved since AR5 (medium confidence), and larger ensembles allow a better quantification of uncertainty in projections due to internal climate variability." (Arias et al., 2021)

Erroneous characterization of the IPCC internal variability range as "peak-to-trough". The report notes (p. 86) that the IPCC assesses the likely contribution of internal variability to long-term warming to be ±0.2°C. The authors incorrectly identify this as a "peak-to-trough" impact of internal variability, implying through this language that up to 0.4°C of the warming signal could be due to internal variability. However, the IPCC number means that they assess internal variability to explain *at most* 0.2°C of the observed warming, while finding it equally likely that it made the observed warming 0.2°C lower. The correct interpretation is that, had the phasing of the internal variability been different, the observed warming could have been up to 0.2°C *lower or higher*.

Misleading statement about relative role of internal variability vs. anthropogenic forcing over the last centuries. The report states that "Over many centuries, any global temperature changes from the troughs and peaks will cancel out, with little to no net impact." This statement ignores what we definitively know about global temperature over the last many centuries from paleoclimate approaches: that late 20th century global warming exceeds the full range of global temperature variations from 1000-1800CE (Pages2K Consortium, 2019). Authors are encouraged to include a graph of reconstructed global temperatures, plotted with instrumental global temperatures (such as that shown below from the PAGES2K Consortium, 2019), in addition to the instrumental global temperature graphs they include.

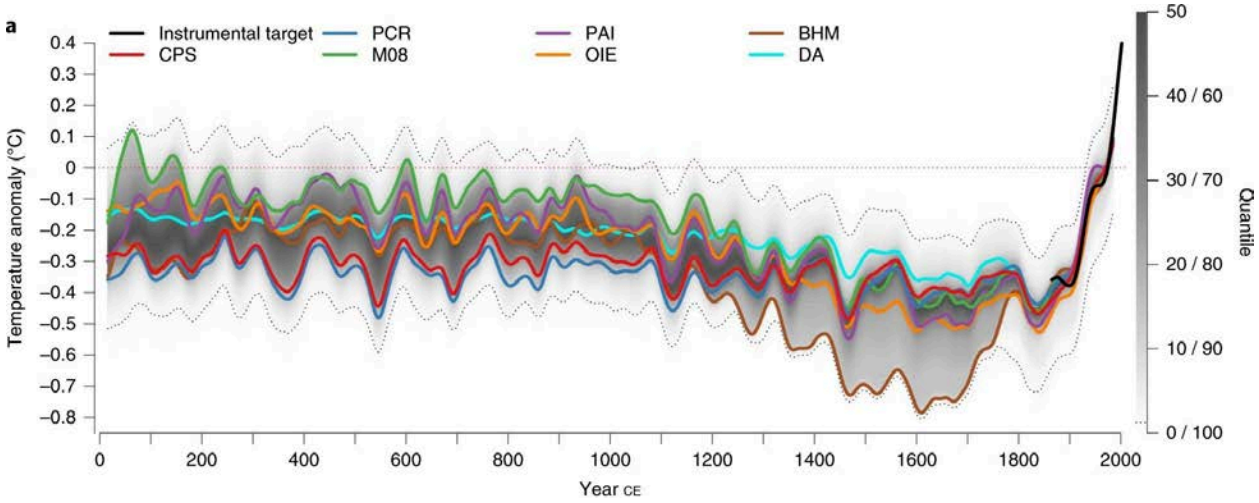

Figure caption from Figure 1 of PAGES2K Consortium, 2019: Global mean surface temperature anomalies with respect to 1961–1990 CE. The coloured lines represent 30 yr low-pass-filtered ensemble medians for the individual reconstruction methods. The grey shading shows the quantiles of all reconstruction ensemble members from all seven methods (right-hand y axis); the 2.5th and 97.5th percentiles are indicated with black dotted lines. The black curve is instrumental data for 1850–2017 CE from Cowtan and Ray, 2014.

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Response to Section 8.3.1

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This section is a **technical and editorial comment** on Section 8.3.1 “Solar Variability” of the DOE CWG report, beginning on **page 84 and continuing until page 85.**

In this section of the DOE report, the authors state on page 84, “However, the recommended forcing dataset for the CMIP6 climate model simulations used in AR6 for attribution studies averages two data sets with low solar variability (Matthes, 2017).” This is used to argue that it is possible for solar variability to be causing a significant amount of the modern warming.

Response:

First, the citation is not correct, it should be (Matthes **et al.**, 2017). This is the paper that provided the solar forcing timeseries for all CMIP6 model simulations (IPCC, 2021) (in the AR6).

Solar variability since 1850 and over the past millenia

It is correct that the long-term trend in TSI from Matthes et al. (2017) is on the low variability side, but this argument ignores that TSI data sets with more extreme trends (high variability trends; Lean et al., 1995 and Shapiro et al., 2011) are thought to be unrealistic (Wang and Lean, 2021) and are hence not used in model intercomparison projects such as PMIP3 (see Figure 1, blue curve). The IPCC (2013) states that “most recent reconstructions show a considerably smaller difference (<0.1%) in TSI between the late 20th century and the Late Maunder Minimum (1675–1715) when the sun was very quiet, compared to the often-used reconstruction of Lean et al. (1995) (0.24%) and Shapiro et al. (2011) (~0.4%).”

In addition, IPCC (2021) explains that millennial-scale changes in TSI are within typical magnitudes of 1.5 [1.4 to 2.1] W m^2 (C.J. Wu et al., 2018) and though extreme changes cannot be ruled out completely (Egorova et al., 2018; Reinhold et al., 2019), these likely did not happen over the past 9 kyr (see also Figure 2).

The statement, “However, the recommended forcing dataset for the CMIP6 climate model simulations used in AR6 for attribution studies averages two data sets with low solar variability (Matthes, 2017)” from the DOE report is also misleading since the CMIP6 model simulations for IPCC start in 1850 and hence do not include the Maunder Minimum (1645-1715). Therefore the (more extreme) secular trend of solar variability is not relevant for the period starting in 1850.

In Matthes et al. (2017) we provide a Maunder-Minimum-like case as an extreme future scenario and sensitivity experiment (see below for more details on this).

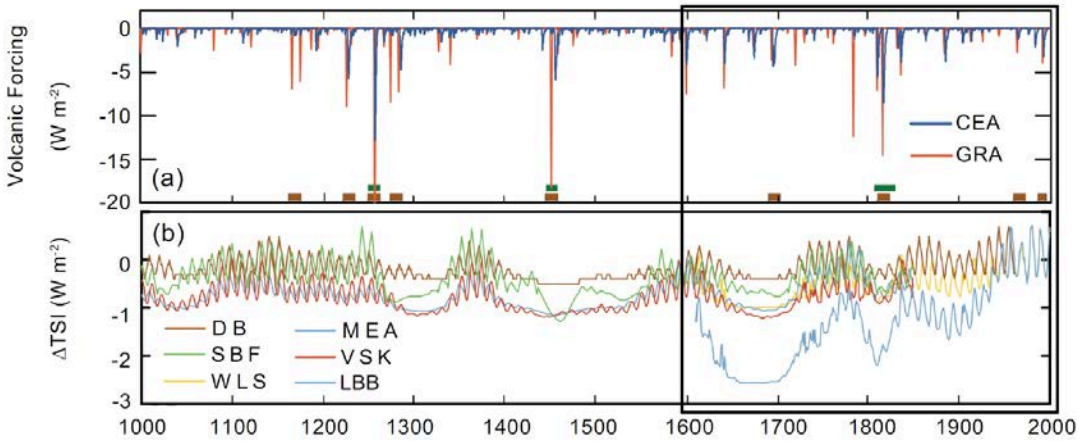

Figure 1: *Overview of solar and volcanic forcing used in the Past Model Intercomparison Project 3 (PMIP3) simulations in support of the IPCC (2013) (AR5). All records except LLB (Lean et al., 1995) have been used in PMIP3 simulations. (Fig. 5.1 a,b from IPCC, 2013).*

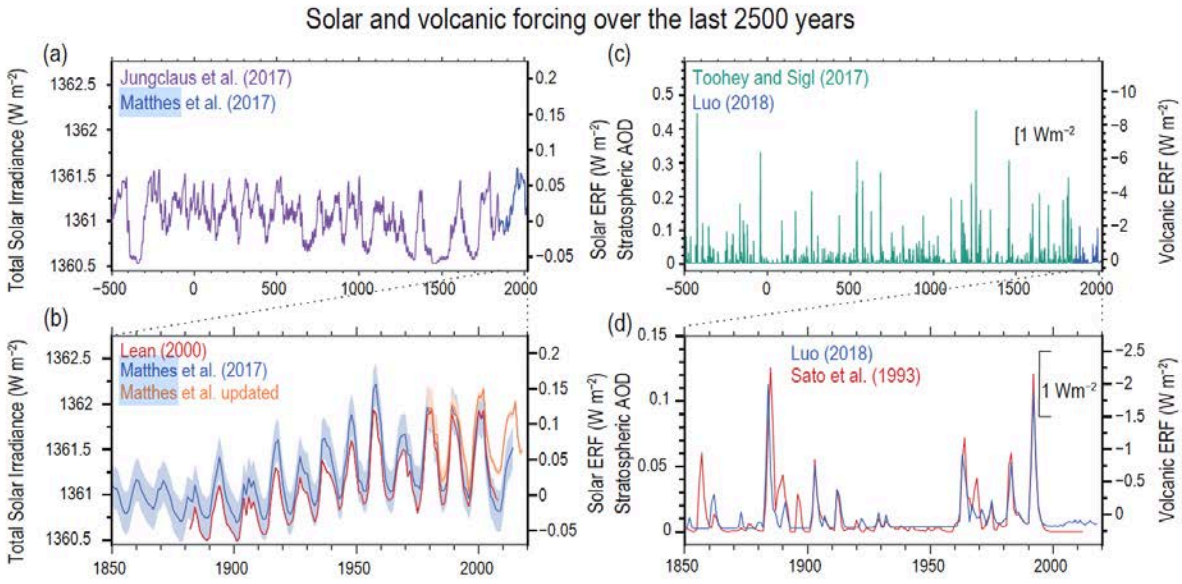

Figure 2: *Overview of solar and volcanic forcing for the last 2500 years (a, c) and since 1850 (b,d) used in the Past Model Intercomparison Project 4 (PMIP4) simulations in support of the IPCC (2021) (AR6). (Fig. 2.2 from IPCC, 2021).*

Solar variability over the satellite period

The four paragraphs on page 85 of the DOE report beginning with "While AR6 shows..." are misleading. They talk about the challenging reconstruction of TSI over time, in particular over the satellite period, but they ignore our community's work and efforts to reconstruct TSI and uncertainty estimates (Matthes et al., 2017).

In Matthes et al. (2017), we concluded the following (just as an example of mis-citation):

The sentence in the DOE report, "The choice of TSI satellite record used in an analysis can therefore substantially influence how much climate change is attributed to human versus natural forcings" is unsupported by the scientific literature. Over the last three solar cycles in the satellite era there is a slight negative TSI trend in the CMIP6 dataset (Figure 3).

A recent reconstruction of the TSI, with a proper estimation of its uncertainties, suggests that this downward trend between the solar minima of 1986 and 2009 is not statistically significant (Dudok de Wit et al., 2017). The TSI trend leads to an estimated radiative forcing on a global scale of -0.04Wm^2, which is small in comparison with other forcings over this period. This is true regardless of which TSI dataset is chosen.

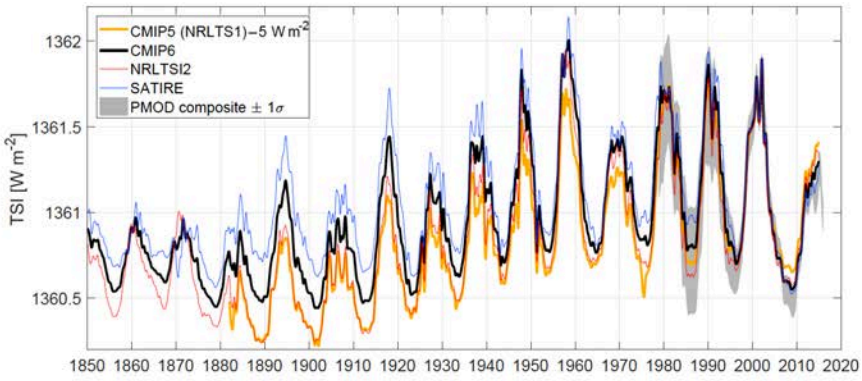

***Figure 3**: Comparison of several TSI solar forcing reconstructions used in CMIP5 (yellow) and CMIP6 (black). Also shown is the basis of the solar forcing CMIP6 reconstruction as well as the observational composite from PMOD with a confidence interval (Fig. 1 from Matthes et al., 2017).*

Impact of a future solar Maunder-Minimum-like period

The statement "However, the recommended forcing dataset for the CMIP6 climate model simulations used in AR6 for attribution studies averages two data sets with low solar variability (Matthes, 2017)" is misleading in another way.

The IPCC model runs do not include the Maunder Minimum period (1645-1715), so differences in the TSI reconstructions during the Maunder Minimum are irrelevant for analysis of the modern period, starting in 1850.

The question that is relevant and important is: what would happen if the Sun would run into a Maunder-Minimum-like period in the future? This would have a cooling effect that

would counteract global warming. There are several studies that analyzed exactly this question (Meehl et al., 2013; Maycock et al., 2015). They consistently show that even a massive decrease of TSI by 0.25% over a 50-year period from 2020 to 2070 (Fig.1 from Meehl et al., 2013; left figure below), imposed in a future climate change scenario experiment, has a small impact on global warming (Fig. 2 from Meehl et al., 2013; right figure below). Once the solar minimum is over, the warming of global surface temperatures catches up to the reference simulation. Please note that the simulated decrease in TSI by 0.25% in Meehl et al. (2013) is much more than the uncertainty range in solar forcing reconstructions for CMIP6 in Figure 3.

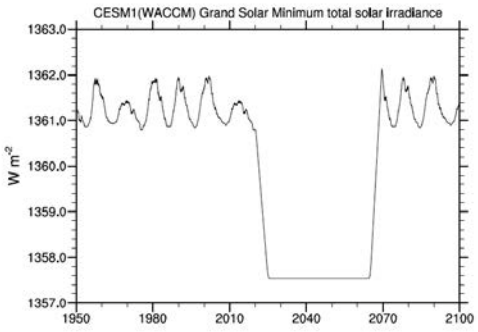

Figure 1. Time series of total solar irradiance input to WACCM showing a reduction of TSI from 2020 to 2070 with maximum reduced values of 0.25% from 2025 to 2065.

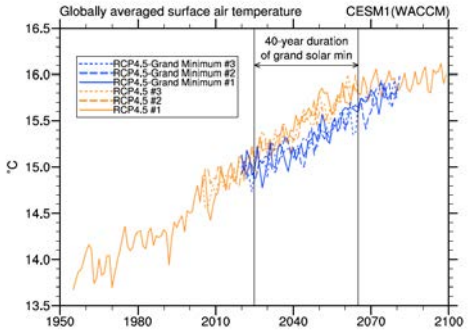

Figure 2. Time series of globally averaged surface air temperature anomalies (°C) relative to the 1986–2005 reference period for the WACCM standard RCP4.5 simulations (orange lines) and the grand solar minimum experiment (blue lines). The duration of the grand solar minimum experiment shown in Figure 1 is indicated from 2025 to 2065.

Solar indirect effects

The next completely wrong statement in the DOE report is on page 85, in the last paragraph in the Solar Variability paragraph: “Such solar indirect effects are not included in climate models, ...”. In Matthes et al. (2017) we provide for the first time ever a comprehensive dataset including TSI and solar spectral irradiance data as well as energetic particle forcing. We also provide an ozone dataset with the indirect effect included.

The solar spectral irradiance data, the energetic particle forcing and the ozone dataset are explicitly provided to include solar indirect effects in CMIP6 models. For the first time in the IPCC, many of the CMIP6 models do include solar indirect effects by using spectral solar irradiance as well as ozone changes. Some CMIP6 models even include particle effects for the first time.

Conclusions

In the above sections on solar variability since 1850 and over the past millennia, solar variability over the satellite period, impact of a future solar Maunder-Minimum-like period, and solar indirect effects, it is demonstrated that sound scientific evidence is misinterpreted or mis-cited, often leaving important details from Matthes et al. (2017)

out. By selecting which facts to amplify, and by citing only one or two papers (e.g., Scafetta et al., 2023) without the context of the full scientific literature and "simplifying it", the conclusions they draw are simply wrong.

Instead, model simulations for AR6 are the most comprehensive set of experiments, including direct and indirect solar effects on climate. The last sentence in the section "Solar Variability" on page 85 "However, claims of non-TSI mechanisms influencing climate are uncertain and debated" is nonsense and demonstrates that the authors of the DOE report did not read the literature, even those papers that they cited (e.g., Matthes et al. 2017). To conclude, we demonstrated that solar variability cannot be causing a significant amount of modern warming.

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IPCC, 2013: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 1535 pp.

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Response to section 8.3.1

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This section is a **technical comment** on Section 8.3.1 of the DOE CWG report, beginning on **page 84**.

Overall, this section overemphasizes uncertainty and claims an inadequate assessment of natural climate variability in IPCC AR6, whereas we do know with high certainty that anthropogenic greenhouse gas emissions have caused the global warming over the last 50-100 years. There are no credible forcings other than greenhouse gases that could have caused that warming.

Temperature measurements in the ocean show that the ocean is warming from the top down, which is inconsistent with a role of ocean circulation in causing atmospheric warming (Barnett et al., 2005; Cheng et al., 2020). Energy conservation requires that, if ocean circulation had caused surface warming, there must be cooling in the subsurface. However, this is not observed. The ocean has warmed at all depth, most at the surface, a pattern that is consistent with anthropogenic warming. This should be mentioned in the part entitled “Natural Variability of Large-Scale Ocean Circulation”, otherwise this part is misleading.

Since AR5, there has been increasing and robust evidence that external forcings—including anthropogenic greenhouse gases and aerosols, as well as natural volcanic aerosols—have modulated the Atlantic Multidecadal Oscillation over the historical period, as assessed in AR6 WGI Section 3.7.7. This important aspect is completely overlooked in Section 8.3.1. Evidence that multi-decadal trends in the Pacific Decadal Oscillation have been anthropogenically forced (Klavans et al., 2025) should also be discussed in this context.

Although state-of-the-art climate and Earth system models tend to underestimate natural internal climate variability, single-model initial-condition large ensembles have provided a pathway to better quantify its range. Using several such ensembles, AR6 WGI Box 4.1 assessed that the 5–95% ensemble range of annual mean global surface

air temperature is approximately ±0.25 °C (MIROC6, IPSL-CM6A), spanning from about ±0.2 °C (CanESM5) to ±0.3 °C (EC-Earth3). This model-based estimate is consistent with observation-based estimates.

The following text on page 88 in this section is therefore misleading: “When the Great Pacific Climate Shift is accounted for in climate attribution analyses since 1950, 40 percent or more of the warming in the second half of the 20th century is attributed to natural internal variability (McLean et al., 2009; Tung and Zhou, 2013; Chylek et al., 2016; Scafetta, 2021)”. There are three problematic aspects in this text. **First, the argument relies exclusively on Tung and Zhou (2013) and is not fully supported by the other three cited references.** Specifically, McLean et al. (2009) and Scafetta (2021) provide no quantitative estimates of the contribution of internal variability to the warming trend. Moreover, the estimate presented in Chylek et al. (2016) is substantially lower than that of Tung and Zhou (2013). According to Chylek et al. (2016), the contribution of the Atlantic Multidecadal Oscillation (AMO) to the 1970–2005 warming was between 0.13 and 0.20 °C (depending on the AMO index used), compared with a greenhouse gas contribution of 0.49–0.58 °C.

Second, while Figure 4 of Tung and Zhou (2013) suggests that internal variability may account for approximately 40% of the observed global warming over the past 50 years, it does not contribute significantly to the long-term warming trend over periods exceeding 75 years. This interpretation is consistent with the AR6 assessment (Section C.1, WGI SPM), which states: “Natural drivers and internal variability will modulate human-caused changes, especially at regional scales and in the near term, with little effect on centennial global warming. These modulations are important to consider in planning for the full range of possible changes.”

Third, the statement does not acknowledge the ongoing debate regarding the role of external forcings in driving AMO variability. Recent studies have highlighted that what has traditionally been interpreted as an internally generated oscillation may, at least in part, reflect externally forced variability, thereby complicating the estimation of the contribution of internal variability to global warming when relying solely on statistical analyses of observational data.

To correct this, the following changes must be made in the DOE CWG document:

- Revise the statements that claim an inadequate assessment of natural climate variability by IPCC AR6. IPCC AR6 lists extensive peer-reviewed literature that led to the following assessment: “When estimated over the entire historical period (1850–2020), the contribution of natural variability to global surface warming of –0.23°C to +0.23°C is small compared to the warming of about 1.1°C observed

during the same period, which has been almost entirely attributed to the human influence."

- Include evidence from ocean warming pattern, which is inconsistent with changes in ocean circulation causing the observed global warming, but which is consistent with anthropogenic warming causing heat entering the ocean from above.
- Include the growing body of robust evidence that external forcings have modulated the Atlantic Multidecadal Oscillation over the historical period, which has important implications for attributing recent trends to natural internal variability.
- Include a more comprehensive assessment of the contribution of natural climate variability to the global warming trend, as in IPCC AR6 reports.

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Section 8.3.1: Solar variability

Dr. Theodosios Chatzistergos[1] and Dr. Ted Amdur[2]

This section is a **technical comment** in response to Section 8.3.1 (pg. 84-85) of the DOE CWG report, specifically the subsection titled “Solar variability.” We, Theodosios Chatzistergos and Ted Amdur, submit these comments in our personal capacity as concerned scientist stakeholders working in academia and industry, respectively. As scientists, we are concerned about the quality and representation of scientific literature in the Climate Working Group report and how such report will be used to inform the Environmental Protection Agency’s (EPA) proposal to repeal the 2009 Endangerment Finding under Section 202(a) of the Clean Air Act (CAA), allowing EPA to prescribe standards for greenhouse gas emissions.[3]

Because of our unique position as experts on solar variability, with specialized knowledge of the academic literature on this topic, we feel compelled to clarify the record’s depiction of the current best available science, which is a cherry-picked subset of the available literature that is evidently selected for their conclusions, rather than the studies’ quality, sophistication, rigor, or prescience. In the following sections, we begin by outlining the overarching flaws in the “Solar variability” subsection of the CWG Report. We then assert a series of rebuttals to specific claims made in the report that are not, and cannot be, substantiated by the current state of the scientific literature.

To correct these technical flaws, the following changes must be made in the DOE CWG report:

- **Review and include best available scientific literature,** specifically literature showing that between Maunder minimum and present, solar influence on Earth's climate has had a net effect far smaller than known anthropogenic effects.
- **Discontinue reliance on verifiably flawed and unrepresentative literature.** The report must discontinue reliance on and remove references to studies known to be invalid and methodologically flawed, due to the use of cherry-picked data, outdated and physically

[1] Dr. Chatzistergos is currently a postdoctoral researcher at the Max Planck Institute for Solar System Research. He earned his Ph.D. from the Georg-August-Universität Göttingen, specializing in the analysis of historical solar observations. His research focuses on elucidating past solar magnetism and, reconstructing long-term solar activity and irradiance variations to better understand their influences on Earth. Dr. Chatzistergos has received awards for his research, including the SCOSTEP Distinguished Young Scientist Award and the Alexander Chizhevsky Medal.

[2] Dr. Amdur is currently an Assistant Director of catastrophe modeling at Aon Impact Forecasting, applying Bayesian statistical methods to the modeling of hurricane risk. Dr. Amdur earned his Ph.D. from Harvard University, where his research and dissertation was devoted to inferring the Sun-climate connection from the past four hundred years of available records. His Sun-climate research was supported by a NASA FINESST grant and applied state-of-the-art statistical methods, including from the econometrics literature, to better interpret the satellite and solar proxy records that are used to reconstruct solar variability

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[3]https://www.epa.gov/newsreleases/epa-releases-proposal-rescind-obama-era-endangerment-finding-regulations-paved-way

implausible models, and statistical methods that have been discredited in the peer-reviewed literature (e.g., Connolly et al. 2021; Stefani 2021). The deliberate inclusion of such unrepresentative, flawed, and discredited studies--while ignoring the vast body of countervailing evidence in available literature--constitutes a failure to use the **best available science** and renders the report's conclusions arbitrary.

- **Accurately contextualize the impact of non-TSI solar forcings based on the weight of evidence.** The report must be revised to accurately represent the current scientific understanding of non-TSI mechanisms, which shows that their influence on global climate is minor. Instead of presenting highly speculative or largely refuted mechanisms (like the link between galactic cosmic rays and clouds) as significant and unresolved debates, the report must include the key **constraining evidence** from major experimental and modeling efforts (e.g., Pierce 2017 on the CERN CLOUD experiment) that place upper bounds on their potential effects. Presenting only the arguments for a large effect, while omitting the strong evidence against it, creates a biased and indefensible record.
- **Acknowledge and incorporate established physical constraints.** The report must be revised to incorporate the known physical constraints on long-term Total Solar Irradiance (TSI) variability, as established in studies such as Yeo et al. (2020). Any discussion of "high variability" scenarios that contradict these established physical upper bounds are fundamentally irrational and must be removed because they rely upon physically unrealistic scenarios.
- **Accurately represent consensus scientific reports.** The text must be corrected to accurately represent the findings of the Intergovernmental Panel on Climate Change (IPCC) Assessment Reports. This includes clearly distinguishing between concepts like Total Solar Irradiance (TSI) changes, Radiative Forcing (RF), and Effective Radiative Forcing (ERF). The misleading claim that IPCC AR6 shows a "substantially greater solar impact" than AR5 must be corrected to reflect that their radiative forcing estimates are, in fact, broadly consistent.
- **Substantiate all claims with correct and relevant citations.** Vague, unsubstantiated phrases such as “growing evidence” must be removed and replaced with specific citations to high-quality, peer-reviewed literature. Furthermore, all citations must be verified for accuracy, correcting erroneous references to non-existent studies, such as “Scafetta et al. (2023)”. Such reliance on non-existent evidence is an arbitrary analysis.
- **Address and resolve known data discrepancies, rather than merely highlighting them.** Rather than presenting historical data disputes such as the "ACRIM-gap" as a major unresolved issue that undermines the entire observational record, the report must incorporate the recent scientific literature (e.g., Amdur and Huybers 2023; Chatzistergos et al. 2025) that has largely resolved this discrepancy. Presenting a resolved issue as a source of major uncertainty is misleading and fails to reflect the best available science.
- **Apply causality tests uniformly.** The report advocates for the use of sophisticated causality tests to evaluate mainstream climate science. It must therefore apply these same tests to the alternative studies it cites (e.g., Scafetta 2013, Stefani 2021). Failing to apply its own stated standards of evidence consistently and even-handedly is definitionally arbitrary.

I. Overarching Flaws

1) An unsubstantiated claim that there exists “inadequate assessment of natural climate variability”

Page: 84
Comment type: Technical

Comment: On page 84 of the DOE CWG report, the authors identify “three areas of substantive criticism of the IPCC’s assessment of the causes of recent warming,” which are the “inadequate assessment of natural climate variability, inappropriate statistical methods, and substantial discrepancies between models and observations.” The report implies that solar variability, as a form of natural climate variability, has been inadequately assessed by the IPCC, calling into question the IPCC’s conclusion that the majority of planetary warming can be attributed to anthropogenic climate change.

We first note the rhetorical sleight of hand being practiced in this section. The DOE CWG report claims it will review the “scientific certainties and uncertainties in how anthropogenic carbon dioxide (CO_2) and other greenhouse gas emissions have affected, or will affect, the Nation’s climate, extreme weather events, and selected metrics of societal well-being.” (pg. ix). While the UN IPCC draws from a wide spectrum of the climate science literature, critiquing the editorial choices of the text within the IPCC reports does not bear upon the state of the science itself. There exists a diverse array of scientific literature to answer the authors’ questions, if they look outside the IPCC reports’ text to the studies cited in the IPCC reports, as well as other published papers. Indeed, much of the perceived lack of discussion of natural climate variability in the IPCC reports is precisely because so much literature has been produced to refute the claims of the DOE CWG report authors, and over so long a time period, that these claims are no longer part of the active scientific debate and are thus given only brief note in the most recent IPCC Assessment reports.

Taken as a whole, this claim of “inadequate assessment” is reminiscent of someone claiming a tool needed for a job doesn’t exist because they failed to find the correct aisle at the hardware store.

In points 3, 4, 5, and 8 of this comment, we will provide specific examples of how questions of natural climate variability, and specifically solar variability, that were raised by the authors have been carefully and thoroughly addressed by recent scientific studies.

2) Inconsistent standards of scientific merit
Pages: 84-90
Comment type: Technical
Comment: In Section 8.3.1 of the DOE CWG report, extensive discussion is made of how natural variability of large-scale ocean circulations, as well as inappropriate statistical methods, call into question the mainstream scientific attribution of planetary warming to anthropogenic versus natural causes. They argue that climate model studies are challenged in resolving multi-decadal fluctuations in ocean circulation that can affect global mean surface temperatures over many decades, and that more sophisticated statistical methods of assessing causality are needed to determine the relative roles of anthropogenic and natural roles in climate change.

Based upon these same standards, however, none of the studies cited in the “Solar variability” section as evidence of an increased role for solar variability in climate would survive scrutiny (Scafetta, 2013; Stefani, 2021; Scafetta et al., 2023 [sic]). While there exist differences in the respective methods of these papers, all three share common characteristics that the DOE CWG report authors regard as methodological flaws in the same section:

1) These statistical studies fail to consider variability of large-scale ocean circulations with any sophistication, either ignoring the contribution (Scafetta, 2013) or making the unsubstantiated claim that using astronomical or solar records to explain global temperatures inherently captures the behavior of ocean circulation variability, as ocean circulation variability is itself controlled by

these extraterrestrial factors (Stefani, 2021; Scafetta, 2023). This circular reasoning begs the question.

2) These studies repeat statistical approaches that have long been known to be methodologically flawed (Richardson and Benestad, 2022), and come nowhere close to the sophistication called for by the DOE CWG report authors for the papers they hold in doubt. Scafetta and co-authors have attempted many statistical undertakings to demonstrate the primacy of solar variability in controlling global temperatures, and while the specifics have varied, those studies, including the two cited here, are subject to the general concerns raised by Benestad and Schmidt (2009) in a careful evaluation of one of the many Scafetta et al. approaches. One objection by Benestad and Schmidt (2009), which is relevant to the three studies cited by the DOE CWG authors, is the following:

> We have also shown through the regression exercises that neglecting important forcings may inflate the climate sensitivity estimates since colinearity between different forcings interferes with the estimation of the sensitivity to each other.

To restate this concern in plain terms, records of solar variability can share the same trends as other variables that affect climate, including greenhouse gas emissions and aerosol emissions, thus a statistical analysis such as regression that inputs each variable without any context is liable to confuse the attribution of temperature between true causes and lookalikes. All three studies noted here from this subsection of the DOE CWG report perform a form of regression analysis that do not include all relevant variables (such as ocean circulation-induced variability, as noted in point A), and thus are at risk of this statistical error. Ironically, these studies also fail to validate their claims by employing the causality tests that are advocated for by the authors just a few pages later in 8.3.1 (pg. 88 - 90).

When high standards are held for a subset of scientific studies, while those exact same standards are not held for another subset, and when the only consistent distinguishing feature between the two subsets appears to be the normative direction of the studies' findings (broadly supporting or refuting the salience and impact of anthropogenic climate change), we are led to conclude that this section begs the question and uses a cherry-picked, distorted view of the available literature to reach the stated conclusions.

II. Specific Rebuttals

3) The authors cite studies that rely upon solar variability scenarios that have been demonstrated to be physically unrealistic
Page: 85
Comment type: Technical
Comment: In the following passages on page 85 of the DOE CWG report, the authors make claims that require there to exist a spectrum of potential histories of solar variability, from low to high:

> While AR6 shows a substantially greater solar impact than does AR5, the overall impact of solar forcing on the climate was still assessed to be small compared to anthropogenic forcing. However, the impact of solar variations on the climate is uncertain and subject to substantial debate (Lockwood, 2012; Connolly et al., 2021) - something that is not evident in the IPCC assessment reports.
>
> This uncertainty causes some reconstructions of TSI from 1750 to have low variability (implying a very low impact of solar variations on global mean surface temperature) whereas datasets with high TSI variability can explain more than 70 percent of the temperature variability since preindustrial times (Scafetta, 2013; Stefani, 2021). The choice of TSI satellite record used in an

analysis can therefore substantially influence how much climate change is attributed to human versus natural forcings.

This narrative, however, is invalidated by the scientific reality that the high solar variability scenarios cited here have been shown to be physically unrealistic. In a 2020 paper titled “The Dimmest State of the Sun”, Yeo et al. establish an upper bound on how much total solar irradiance (TSI) could have varied between now and the Maunder Minimum, finding an upper bound limit of TSI change of **2.7 W/m²** through a physical model constrained by state of the art solar imagery and the known physics of the Sun. It is important to note that the Yeo et al. 2020 constraint is about the dimmest possible state of the Sun without specific claim for the Maunder minimum and it is very likely that Maunder minimum did not reach this dimmest state. Separately, Rempel (2020) finds that even if the quiet-Sun/small-scale magnetic field could vary in ways that have been unobserved, the high variability scenarios used in the papers cited by the DOE CWG report would be “unrealistic” by requiring a 50% reduction in the quiet magnetic field strength in the past.

It is a straightforward misrepresentation for the authors to elide these studies when discussing solar variability scenarios, as they provide a straightforward explanation for why a higher degree of solar variability is not discussed extensively in the most recent IPCC assessment report.

4) The studies cited by the report authors suffer from critical methodological flaws

Page: 85
Comment type: Technical
Comment: In this comment, we detail the critical methodological flaws exhibited by two papers cited by the DOE CWG report authors as evidence of past solar variability large enough to explain the majority of the historical global temperature record and as evidence that such a causal explanation of global temperatures due to solar variability can be inferred from statistical study.

We begin with Connolly et al. (2021), which provides demonstration of how the high solar variability scenarios, in contradiction to known physical constraints, are constructed from flawed methodologies.

Connolly et al. 2021 cherry-picked a set of 16 TSI series, deliberately balancing the sample to include eight with low long-term variability and eight with high variability. To achieve this equal division, they included outdated or superseded versions of some models, while they ignored many low variability TSI reconstructions. We note this cherry-picking in Chatzistergos et al. (2023), which states: “often older versions are used in the literature ignoring the fact that newer versions, overriding the earlier ones, are available. We caution against using outdated reconstructions.” Connolly et al. 2021 did not clarify how the division between high and low variability series was done, but all high variability series they used lie outside the constraint imposed by Yeo et al. (2020) that there does not exist a physical basis for assuming that TSI variability can exceed **2.7 W/m²** versus present day.

The high variability group in Connolly et al. 2021 includes:

- **Hoyt and Schatten (1993)** The Hoyt and Schatten (1993) model has been shown to be highly unrealistic and is now widely discredited, as discussed in detail in our prior comment (Chatzistergos, 2024). As detailed in Chatzistergos (2024), it contains multiple arbitrary adjustments and fabricated data for one index spanning over a decade, apparently copied from another index. Crucially, its high variability was imposed by the authors rather than emerging from the model itself. When updated sunspot indices for updating the model and direct TSI composites

(e.g., PMOD, Montillet et al. 2022, ACRIM) are used, it becomes clear that Hoyt and Schatten (1993) exaggerated TSI variation amplitudes by roughly a factor of five.

- **Lean et al. (1995)** An early and now outdated version of the NRL model. It has long been superseded by newer versions, latest by NNL.
- **Bard et al. (2000)** reconstructed TSI by applying a simple linear scaling to the production rates of cosmogenic isotopes (e.g., ^{10}Be), without accounting for nonlinear effects such as geomagnetic modulation, (Usoskin 2023). More recent and physically realistic models, such as **SATIRE-M** (Wu et al. 2018), explicitly incorporate this and indicate substantially lower long-term TSI variability. Furthermore, Delaygue and Bard (2011) revised the original Bard et al. (2000) reconstruction downward, but Connolly et al. (2021) ignored this update.
- **Shapiro et al. (2011)** an older version of Egorova et al. 2018
- **Egorova et al. (2018)** Connolly et al. 2021 included this model four times (five considering the Shapiro et al. 2011 too) as distinct reconstructions. The Egorova et al. (2018) model relies heavily on assumptions about the Sun's quietest possible state, a critical and uncertain input. This approach has been strongly criticized by Yeo et al. (2020), who demonstrated that the long-term TSI variability in the Egorova et al. 2018 model is exaggerated.

Over recent decades, our irradiance modeling capabilities have improved dramatically. For example, Shapiro et al. (2017) and Yeo et al. (2017), using the state-of-the-art physics-based modeling in the SATIRE-3D model, demonstrated that we can reproduce TSI measurements with exceptional accuracy across all measured timescales without the need for any adjustments or free parameters in the model. Thus, there is no valid motivation for an arbitrary adjustment to a solar variability reconstruction due to a model-data mismatch. Additionally, there have been significant advances in the analysis and compilation of solar data (Clette et al. 2023; Chatzistergos et al. 2024) needed for irradiance reconstructions. These improvements, along with enhanced modeling and new high precision direct irradiance measurements (Kopp 2025), have significantly advanced our understanding of solar irradiance variability. The current consensus is that the secular trend in irradiance variations is relatively small. The extent of this trend is further constrained by Yeo et al. (2020), a study that, as mentioned in the prior comment, established a robust constraint on the Sun's dimmest state, the lowest expected TSI value, to be **2.7** W/m² below the 2019 minimum.

An additional important point is that in all 8 high variability models shown by Connolly et al. 2021, the long-term TSI variability was not derived from observational constraints but was instead imposed as a model assumption. Modern updates of the NRL model (Coddington et al. 2019; Lean 2018) and Hoyt and Schatten 1993 (Chatzistergos 2024) have shown that the high variability that was imposed in the earlier versions is inconsistent with direct TSI measurements and have therefore been revised to show lower variability. Richardson and Benestad (2022) have also criticized the methodology used by Connolly et al. (2021) to attribute temperature changes to TSI.

Another point about the misleading presentation of our current understanding on long-term changes in irradiance made by Connolly et al. (2021) was presented by Chatzistergos et al. (2023). Chatzistergos et al. (2023) collected most (if not all) available reconstructions of total solar irradiance (TSI) that cover the period 1700-1986, grouping different versions of individual models together. They found 24 groups of models, out of which 21 suggest a rather small secular trend (here defined as difference between activity minima in 1986 and 1700) that is between −0.09 and 2.2 Wm^{-2}. Only 3 groups of models were found to exhibit a high secular trend (Hoyt and Schatten 1993; Egorova et al. 2018; Fligge and Solanki 2000). As noted in Chatzistergos et al. (2023), the study by Fligge and Solanki 2000 is "considered replaced by the newer SATIRE-family models".

Overall, despite the uncertainty in estimating the secular trend in TSI variations, current models suggest a relatively weak secular trend, and all high variability models have been criticized or even rendered invalid (in the case of Hoyt and Schatten (1993)). While old versions of models that were at some point suggesting high secular trends, have been typically revised and now exhibit low secular trends. Connolly et al. (2021) significantly overstates the Sun's role in recent climate change by selectively including outdated, superseded, and implausible TSI reconstructions that are inconsistent with modern observational and theoretical understanding. Chatzistergos et al. 2023 also states: "often older versions are used in the literature ignoring the fact that newer versions, overriding the earlier ones, are available. We caution against using outdated reconstructions."

Turning to Stefani (2021), we do not believe this paper represents the best available science when it includes fundamental methodological flaws that were directly identified in the Sun-climate literature decades ago (Pittock, 1978; Benestad and Schmidt, 2009). We outline three critical flaws in the Stefani (2021) study.

First, they create a hypothesis to fit the facts. We quote extensively from Pittock (1978), a review paper that responds to Sun-climate papers from 50+ years ago with the same flaws seen in Stefani (2021): "One of the greatest temptations when one is faced with facts which do not fully support a hypothesis is to elaborate the hypothesis so as to fit the facts." Here, Stefani (2021) explicitly chose to associate the geomagnetic aa-index with global sea surface temperatures not because that is the most logical predictor between the Sun and Earth's climate, but because it presents a strong match between a solar record and certain Earth temperature records. Pittock (1978) states the danger of such an approach: "The problem with this sort of reasoning is that almost any hypothesis can be made to fit a given set of observations if it is elaborated until it contains enough degrees of freedom. If this is done after an examination of the observational data, the new complex hypothesis can only be tested by comparing new qualitative or quantitative predictions against new or independent data".

Second, they performed a regression analysis without all relevant data. Stefani (2021) uses only two datasets to attempt to predict global temperatures: the geomagnetic index, and a version of the CO_2 record. As we noted in our second comment, this approach has been thoroughly discredited by Benestad and Schmidt (2009):

> We have also shown through the regression exercises that neglecting important forcings may inflate the climate sensitivity estimates since colinearity between different forcings interferes with the estimation of the sensitivity to each other.

To restate this concern in plain terms, records of solar variability can share the same trends as other variables that affect climate, including greenhouse gas emissions and aerosol emissions, thus a statistical analysis such as regression that inputs each variable without any context is liable to confuse the attribution of temperature between true causes and lookalikes.

Third, Stefani (2021) applies smoothing--through the use of a moving average--to their regression analysis without proper consideration of the statistical thorniness that this move entails. Again, this approach is a known pitfall for statistical analyses in the Sun-climate literature that was expounded upon by Pittock (1978) almost 50 years ago as leading to an inordinate number of false positives:

> ...smoothing of climatological series leads to a significant reduction in the number of degrees of freedom applicable to computed correlations between climatological and sunspot data. This leads

> to much greater variability in the computed correlation coefficients, more frequent high values occurring by chance.

We acknowledge that if these two citations are removed, the DOE CWG authors could replace them with entries from a subset of the peer-reviewed literature that would conclude nearly identical findings. These examples of flawed methodology should be understood as illustrations of a recurring **pattern of error**—including cherry-picking, violating physical constraints, and improper statistics—found in a specific subset of the Sun-climate literature. Substituting these citations with functionally identical papers would demonstrate that the report's selection process is predisposed to a particular conclusion, rather than an objective assessment of evidence. A credible revision must actively screen for and exclude studies exhibiting this pattern of flawed methodology.

5) Non-TSI mechanisms of the Sun affecting climate have been studied in depth and are unnecessary to explain the observational record

Page: 85
Comment type: Technical
Comment: The DOE CWG report concludes the “Solar Variability” section with the following paragraph:

> There is growing evidence that other aspects of solar variability, which are referred to as solar indirect effects, either amplify TSI forcing or are independent of TSI forcing. Scafetta et al. (2023) suggests that ~80 percent of solar influence on climate might stem from non-TSI mechanisms. There are numerous candidate processes, including solar ultraviolet changes; energetic particle precipitation; atmospheric-electric-field effect on cloud cover; cloud changes produced by solar-modulated galactic cosmic rays; large relative changes in the magnetic field; and the strength of the solar wind. Such solar indirect effects are not included in climate models, although indirect methods of estimating their impacts suggest they are significant. However, claims of non-TSI mechanisms influencing climate are uncertain and debated.

Nearly every claim in this paragraph is without citation and unsubstantiated. This includes claims of “growing evidence” of the importance of solar indirect effects and that “indirect methods of estimating their impacts suggest they are significant”. Additionally, many of these claims are imprecise to the point that, despite our expertise, we cannot make clear sense of the claim. For example, “large relative changes in the magnetic field” could refer to any number of variations in either the solar or Earth magnetic field. We also struggle to be sure of how to respond to the only citation provided, as there exists no “Scafetta et al. (2023)” paper, and in this comment we surmise that the authors intended to refer to Scafetta (2023), which we respond to below.

That said, we can provide expertise to respond to the claims of the authors. Amdur et al. (2021) was a study undertaken to test the claim that “solar indirect effects are not included in climate models, although indirect methods of estimating their impacts suggest they are significant.” Amdur et al. agreed with the authors that it would be important to understand whether or not climate models are missing an important control on the climate system, and at the time there appeared to be some difference between the magnitude of climate model response to solar variability versus the response to solar variability in the observed temperature record. Through an extensive statistical analysis of existing methods for estimating the indirect impact of solar variability, as well as a thorough methodological approach to compare models and data under 48 combinations of observed datasets, they concluded that, “whereas additional

processes have been proposed to amplify the climate response to solar cycle forcing, observations of the global temperature response to solar forcing are found to be in keeping with standard general circulation model simulations" and that the entirety of the observed differences could be ascribed to quirks with the underlying data or statistical methods.

Importantly, other independent approaches to statistically estimating model versus observed response to solar variability have reached the same general finding (Misios et al. 2016, Li and Tung 2023), and we are unaware of approaches with a similar level of statistical sophistication finding a demonstrable difference between model and observation, let alone the 5x mismatch between modeled and observed climate response to solar variability implied by the DOE CWG report authors and Scafetta (2023), which notably includes responses to solar cycle variability that are far out of line with the findings of Misios et al. (2016), Amdur et al. (2021), and Li and Tung (2023).

Given the inherent agreement observed between models and observations, the "numerous candidate processes" listed by the authors are solutions in search of problems. Many of these processes have been thoroughly vetted and excluded individually, however, making their repetition here a "zombie argument" that is inconsistent with the state of the best available science. For example, the effect of cosmic rays on cloud formation has been shown to have negligible effects based on the CLOUD experiment at CERN (Pierce 2017). Effects on Earth's atmosphere for some of the other processes have been presented in the literature, e.g. Mironova et al. 2015 and Sinnhuber & Funke 2020, although their effects are not found to be as significant relative to solar irradiance variations as implied in the DOE CWG report.

6) Confusion between range of temperature changes due to natural forcings and net effect;
Page: 84
Comment type: Technical
Comment: The DOE CWG report states:

> AR6 states that natural external drivers since 1850-1900 have changed global surface temperature by –0.1°C to +0.1°C, and internal variability has changed it by –0.2°C to +0.2°C – on average having essentially no net impact on the warming since 1850-1900.

The AR6 WGI report presents this in Fig. SPM.2 as "Aggregated contributions to 2010–2019 warming relative to 1850–1900." This figure refers to the net effect of various climate drivers on global surface temperature between these two periods, not the maximum changes observed. The accompanying range represents the uncertainty in these estimates. It is therefore misleading to claim that AR6 asserts natural drivers have changed global temperature by ±0.1°C since 1850–1900. In reality, Fig. SPM.1 shows that natural variability has led to global temperature fluctuations of up to ±0.5°C on annual resolution, but the net contribution of natural drivers to long-term warming is estimated at around ±0.1°C.

7) Confusion between radiative forcings in AR5 and AR6
Pages: 84-85
Comment type: Technical
Comment: The authors of the DOE CWG report potentially conflate two related but distinct concepts: radiative forcing and TSI differences, which were both mentioned in section 8.3.1 but not clearly distinguished.
The relevant text in the DOE CWG report is the following:

> AR5 concluded that the best estimate of radiative forcing due to Total Solar Irradiance (TSI) changes over the period 1750–2011 was very small (0.05 W/m2, Myrhe et al., 2014). AR6 acknowledges substantially higher values and a much larger range of estimates of changes in

> TSI over the last several centuries, stating that the TSI between the Maunder Minimum (1645–1715) and the second half of the twentieth century increased by 0.7–2.7 W/m2, a range that includes both low and high variability TSI data sets (Gulev, 2021). However, the recommended forcing dataset for the CMIP6 climate model simulations used in AR6 for attribution studies averages two data sets with low solar variability (Matthes, 2017). While AR6 shows a substantially greater solar impact than does AR5, the overall impact of solar forcing on the climate was still assessed to be small compared to anthropogenic forcing. However, the impact of solar variations on the climate is uncertain and subject to substantial debate (Lockwood, 2012; Connolly et al., 2021) - something that is not evident in the IPCC assessment reports.

However, AR6 WGI report (page 958) states:

> In contrast to AR5, the solar Effective Radiative Forcing (ERF) in this assessment uses full solar cycles rather than solar minima. The pre-industrial TSI is defined as the mean from all complete solar cycles from the start of the 14C SATIRE-M proxy record in 6755 BCE to 1744 CE. The mean TSI from solar cycle 24 (2009–2019) is adopted as the assessment period for 2019. The best estimate solar ERF is assessed to be 0.01 W m–2, using the 14C reconstruction from SATIRE-M, with a likely range of –0.06 to +0.08 W m–2 (medium confidence). The uncertainty range is adopted from the evaluation of Lockwood and Ball (2020), who performed a Monte Carlo analysis of solar activity from the Maunder Minimum to 2019 using several datasets, resulting in an ERF range of –0.12 to +0.15 W m–2. The Lockwood and Ball (2020) full uncertainty range is halved since the period of reduced solar activity in the Maunder Minimum had ended by 1750 (medium confidence).

By comparison, the AR5 WGI report (page 689) states:

> The best estimate from our assessment of the most reliable TSI reconstruction gives a 7-year running mean radiative forcing (RF) between the minima of 1745 and 2008 of 0.05 W m–2. Our assessment of the range of RF from TSI changes is 0.0 to 0.10 W m–2.

Thus, the solar radiative forcing estimates in AR5 and AR6 are broadly consistent, with AR6's best estimate slightly lower, while its uncertainty including also negative values that AR5 did not. The authors of the DOE CWG report misused a different part from the AR6 WGI text to suggest that AR6 “shows a substantially greater solar impact than does AR5”, which is incorrect.

This claim arises due to AR6 WGI (page 297) listing a TSI difference between the Maunder Minimum and the second half of the 20th century ranging from 0.7 to 2.7 W/m². The 0.7 W/m² lower bound likely corresponds to the SATIRE-T reconstruction, while the upper bound comes from the Yeo et al. (2020) constraint on the dimmest possible solar state. It is important to note that Yeo et al. (2020) does not reconstruct TSI during the Maunder Minimum but rather provides a theoretical constraint on the minimum plausible solar irradiance. This constraint by Yeo et al. 2020 should not be interpreted as an actual historical TSI difference between the Maunder Minimum and the present, but rather as a physical upper bound on the maximum plausible difference between the Sun's dimmest possible state and its current irradiance. Importantly, the Maunder Minimum is unlikely to represent this dimmest state, and modern TSI reconstructions suggest a substantially smaller long-term trend than what a 2.7 W/m² Maunder-to-present difference would imply. This upper limit, derived from Yeo et al. (2020), effectively excludes high-variability TSI reconstructions that exceed this threshold, such as Egorova et al. (2018). Almost all current TSI reconstructions indicate significantly smaller long-term TSI trends (Chatzistergos et al. 2023). These include the SATIRE and NRL (now NNL) models recommended for CMIP6 and AR6.

8) ACRIM-gap
Page: 85
Comment type: Technical
Comment: The DOE report exaggerates the significance of the ACRIM-gap. The delay in the launch of ACRIM2 led to the necessity of using data from Nimbus7-ERB and ERBE-ERB. Nimbus7-ERB is the oldest TSI measuring instrument we have, while ERBE-ERB has lower quality and took observations more sporadically than Nimbus7-ERB. The whole ACRIM-gap issue originates in unaccounted sensitivity changes in either or both of those missions. Nimbus7-ERB suggested a slight increase in TSI between 1990-1992, while ERBE-ERB did not.

While a subset of literature has argued for decades that, through a certain interpretation of the ACRIM-gap, solar irradiance changes since 1978 are unknown or upwards, we have independently led two scientific studies that demonstrate a common understanding of the trends in solar irradiance since 1978, regardless of one's chosen interpretation of the ACRIM-gap.

First, Amdur and Huybers (2023), followed by Amdur and Huybers (2025), developed an independent statistical analysis using a sophisticated Bayesian approach inspired by the time series econometrics literature. In these studies, with clearly defined priors that make no *a priori* assumptions regarding the relative merits of using a given satellite or solar proxy in filling the ACRIM-gap, they find that the best estimate of Total Solar Irradiance (TSI) using almost all available observational satellite data supports a downward trend in TSI since 1978, in line with most reconstructions of irradiance.

Second, Chatzistergos et al. (2025) performed irradiance reconstruction with the physics-based semi-empirical model SATIRE-S using datasets that span the entire ACRIM-gap period and extend beyond it. This modeling approach returns long-term TSI trends that are independent of direct TSI measurements, although some uncertainty remains due to the quality of the input data. By applying the model separately to two independent datasets, Mt. Wilson magnetograms and reconstructed unsigned magnetograms from Meudon Ca II K observations, Chatzistergos et al. (2025) demonstrated that both reconstructions contradict the TSI increase suggested by some during the ACRIM-gap, providing independent evidence against that upward trend.

Thus, while the issues leading to the ACRIM-gap contribute to the uncertainty in the long-term trends in direct TSI measurements, both statistical and physics-based reconstructions relying upon independent data find a common resolution to the ACRIM-gap, and it is important to note that these findings are consistent with existing TSI composites that do not exhibit substantial disagreement (Kopp 2025). In particular, (Chatzistergos et al. 2023) in their Table 1 lists the trend between activity minima of cycle 25 (2019) and 22 (1986) and shows that it is rather small ranging between -7.6 mW m^{-2} y^{-1} (Montillet et al. 2022) and 1.8 mW m^{-2} y^{-1} (Scafetta 2023) (the latter is an informal update of ACRIM TSI composite). Most recently, Amdur and Huybers (2025) found a trend of -5 mW m^{-2} y^{-1} over this period.

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Response to section 8.3.3

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This section is a **technical comment** on Section 8.3.3 of the DOE CWG report, beginning on **page 89**.

In the second paragraph the authors write "An application to the well-known Vostok ice core data revealed an error in Al Gore's documentary An Inconvenient Truth. Gore showed the data and drew attention to the coherence of temperature and CO2 changes over a 440,000 year span, which he asserted was due to CO2 driving temperature changes. But temperature changes can also affect atmospheric CO2 levels. Davidson et al. (2015) examined the series and found that temperature Granger causes CO2 but not the reverse. In other words on the time scales represented in the Vostok data, the coherence in the series is primarily due to the influence of temperature on CO2 levels, not the feedback of CO2 levels on temperature."

This section is contrary to the vast majority of paleoclimate literature, which has established that atmospheric CO_2 has an important influence on global temperatures during past ice-age cycles. For example, radiative forcing of CO_2 and ice sheets similarly contribute to colder temperatures during the Last Glacial Maximum (Broccoli, 2000; Broccoli & Manabe, 1987; Hansen et al., 1984; Köhler et al., 2010; Schmittner et al., 2011; Weaver et al., 1998) and simulations of glacial cycles with constant CO_2 underestimate ice volume changes (Abe-Ouchi et al., 2013; Ganopolski & Calov, 2011). This should be mentioned here, since otherwise it will leave the reader with the false impression that CO_2 variations are unimportant for ice age climate changes.

This section should also clarify that the Vostok ice core data are oxygen isotopes, a proxy for regional temperature in Antarctica. They do not represent global temperature. Using a reconstruction of global temperature over the last deglaciation (last 20,000 years) Shakun et al. (2012) find that CO_2 leads temperature and thus likely causes much of the warming over that time period. This should be mentioned here.

In the first paragraph there are several non-peer reviewed citations, e.g. Razzak (2021) and Dagsvik and Moen (2023). The Razzak (2021) paper is problematic because it removes the trends in both CO_2 and temperature data and then investigates the residuals, finding no correlations using only statistical methods. However, physical understanding of Earth's radiation budget clearly suggests that the trend in CO_2 causes the observed temperature trend (Pierrehumbert, 2011). Thus, removing the trends in both time series is not appropriate for attribution studies. Razzak (2021) also ignores all other known influences on temperature such as internal variability (e.g. ENSO) and other external forcings such as aerosols. I suggest removing Razzak (2021) and other non-peer reviewed citations. Studies that include other forcings find convincing effects of CO_2 on global warming over the last 100 years (e.g. Lean and Rind, 2009).

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Technical Comment on the DOE CWG report, Section 8.4: Declining planetary albedo and recent record warmth

Robert Kopp, Rutgers University; Andrew E. Dessler, Texas A&M University

This is a **technical** comment on section 8.4 of the DOE CWG report (pp. 90–92), and its relation to section 4 of the DOE CWG report (pp. 25–30).

This section highlights the role of declining planetary albedo in recent record global warmth. Much of it summarizes Goessling et al. (2024) and Hansen and Karecha (2025).

Goessling et al. (2024) nicely states the conundrum this section addresses:

> In 2023, the global mean temperature soared to almost 1.5 kelvin above the preindustrial level, surpassing the previous record by about 0.17 kelvin. Previous best-guess estimates of known drivers, including anthropogenic warming and the El Niño onset, fall short by about 0.2 kelvin in explaining the temperature rise. Using satellite and reanalysis data, we identified a record-low planetary albedo as the primary factor bridging this gap. The decline is apparently caused largely by a reduced low-cloud cover in the northern mid-latitudes and tropics, in continuation of a multiannual trend. Further exploring the low-cloud trend and understanding how much of it is due to internal variability, reduced aerosol concentrations, or a possibly emerging low-cloud feedback will be crucial for assessing the present and expected future warming.

As the quote indicates, this is actually a small mystery — out of ~1.5°C of warming, about ~0.2°C is unexplained. One could reasonably look at this as an indication of how successful climate science is that we find such small differences notable.

When considering the importance of events like this, it's useful to remember that the climate system is always doing unusual things. For example, in the 2000s, climate skeptics were laser focused on the so-called "hiatus", a supposed lack of warming in the surface temperature record between the late 1990s and early 2010s. Some people claimed this falsified mainstream climate science.

But, as climate scientists always said would happen, the warming returned with a vengeance in the mid-2010s. In hindsight, the hiatus was simply unusual climate variability, likely connected to ocean dynamics (e.g., Kosaka and Xie, 2013), and its occurrence has no serious implications for long-term climate change (England et al, 2015).

What we've experienced in the last two years has also been equally extraordinary although in the other direction. As a result, scientists have worked hard to explain the warming. Potential explanations include:

Aerosols: Aerosols cool the climate by reflecting sunlight back to space. As humans work to reduce air pollution, this cooling mask is being removed, adding additional heat to the climate system. However, estimates of radiative forcing due to reduction in aerosols suggests that this was too small to produce the observed 2023-2024 excess warming (e.g., Gettelman et al., 2024).

Hunga Tonga volcanic eruption: The eruption of the Hunga Tonga volcano in early 2022 dramatically perturbed our atmosphere by injecting enormous amounts of water vapor directly into the stratosphere. However, estimates of radiative forcing show it was too small to be a factor in the 2023-2024 heat (Schoeberl et al., 2022). In fact, we don't even know if the eruption warmed or cooled the climate.

Natural variability + cloud feedback: Recent work (England et al., 2025) has suggested an intriguing possibility: unusually low wind speeds caused the surface ocean mixed layer in the North Atlantic to become very shallow. Because the mixed layer was thin, it warmed rapidly. When combined with the weak winds, the very warm ocean reduced cloud cover. And with fewer clouds, more sunlight reached the ocean surface, further amplifying the warming. While the immediate trigger was low winds due to unforced variability, climate change has been driving a long-term trend toward shallower mixed layers, making extreme events like this more likely.

The DOE CWG report concludes:

> While it is difficult to untangle causes of the recent trend, the competing explanations for the cause of the declining cloud cover have substantial implications for assessing the Equilibrium Climate Sensitivity and for the attribution of the recent warming. An additional 10 years of data should help clarify whether this is a strong positive cloud feedback associated with warming or a temporary fluctuation driven by natural variability. (p. 92)

It is not clear what point the DOE CWG report is trying to make here. If, as they suggest, the albedo decline represents a strong positive climate feedback, then it means that "the lower end of realistic climate sensitivity estimates of 2.3 to 4.7 K may need to be adjusted upward" (Goessling et al., 2024) — meaning that warming will be at the upper end of the model-projected range. Hansen and Karecha (2025), cited by the DOE CWG report, argue for a climate sensitivity of 4.5°C ± 0.5°C (1σ).

If the albedo decline does not indicate a strong positive cloud feedback, then the existing ECS range of 2.5-4°C would remain appropriate. Notably, the report presents no evidence or even an argument suggesting that this event means that the mainstream ECS range might be too high.

This creates a significant inconsistency within the report. The discussion of climate sensitivity in Chapter 4 (pp. 25-35) does not engage with the active research area described in Section 8.4, instead concluding that "Data-driven ECS estimates tend to be lower than climate model-generated values" (p. 25) — a conclusion that appears inconsistent with the albedo evidence presented elsewhere.

The DOE CWG report should be revised to ensure its discussion of Equilibrium Climate Sensitivity in Chapter 4 reflects the emerging science on declining planetary albedo and potential strong positive cloud feedback discussed in Section 8.4.

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Response to Section 8.5. Attribution of Climate Impact-Drivers

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Section 8.5 (pp. 92-94) of the DOE CWG report outlines their perspective on the attribution to anthropogenic forcings of Climate Impact-Drivers (CIDs). This section includes several misunderstandings of what CIDs represent and what is shown in IPCC Table 12.12 as highlighted below.

Comment: Technical

The DOE report pretends that this table is where the IPCC establishes the attributed signal of climate change. For example, they say that this table shows that “few CIDs exhibit an anthropogenic signal sufficient to distinguish them from natural variability.”

In fact, those tests were performed in attribution studies assessed elsewhere in the report. For example, chapter 11 describes how the anthropogenic signal is detected and attributed in extreme heat (Section 11.3.4), heavy precip (Section 11.4.4), etc, and earlier chapters look at water, energy, land, ocean, biosphere and cryosphere detection and attribution. There is also a prominent section on detection and attribution in chapter 11 (Fig 11.4 and accompanying text).

Table 12.12 is a specific assessment of emergence across broad regions that goes far beyond a detection of climate change or attribution to human influence (which is well described in the IPCC’s Chapter 11). Table 12.12 looks specifically at whether the average year now exceeds the historical range of variation (this is what is meant by “emergence”). For example, is the average annual temperature now warmer than the warmest year in the historical period (which would have been affected by natural variability like ENSO cycles).

Simple physical reasoning tells us that fully emerging from the noise of the historical record happens more rapidly for mean conditions than extremes, so you can see in the table that the mean conditions surpass this “emergence” criterion first with extremes increasingly emerging in the coming decades.

For further context, we reproduce below (Figure 1) a LinkedIn post made by one of the coordinating lead authors of chapter 12 that verifies what the author team was thinking in Table 12.12.

Suggested action:

- Fix the errors in this discussion arising from the DOE CWG not understanding what a CID actually is.

Comment: Technical

The DOE CWG report’s Table 8.1 (Page 94) is intended to be a partial reproduction of Table 12.12 of the IPCC Sixth Assessment Report Working Group 1. With the original table, the IPCC authors are seeking to synthesise available studies on CIDs and where there is a body of evidence to support detectable anthropogenic CIDs, this is noted. The IPCC text notes

that a lack of literature inhibits confidence in robust detection in many cases (e.g. “Studies are missing to properly assess S/N emergence for droughts and for wind CIDs.” P. 1855) and takes the approach that only when there is sufficient evidence in the peer-reviewed literature, a detectable signal of emergence is noted. Such CIDs without sufficient evidence are displayed as white cells.

The DOE CWG report misrepresents these white cells to mean “no signal detected” — suggesting that evidence exists of no signal. This substantially changes the interpretation of the Table. The IPCC report explicitly acknowledges there is a lack of peer-reviewed studies on all CIDs of interest so not every white cell means no signal has been detected, it can also mean that there is a lack of relevant analysis in the literature. This misinterpretation in the DOE CWG gives the false impression that analysis of CIDs has failed to identify signals whereas in some cases there is actually no analysis.

This misinterpretation extends to the text in the DOE CWG report. On page 93 the DOE CWG report states “Nonetheless a striking feature of that summary table is how few CIDs exhibit an anthropogenic signal sufficient to distinguish them from natural variability,” thereby indicating that a white cell represents a lack of anthropogenic signal and excludes the case of a lack of comprehensive peer-reviewed literature.

Suggested action:

- Edit the Table 8.1 caption to match the IPCC’s text in describing how white cells should be interpreted.
- Discuss the challenge of a lack of peer-reviewed literature in some aspects of Climate Impact-Drivers the IPCC seeks to cover and edit the text accordingly.

Comment: Technical

Page 93 includes the line “Note that one of the CIDs is an increase in CO_2 levels, and since it is a tautology to attribute this to increased CO_2 levels this CID can be ignored.” This misconstrues what a CID is and even misrepresents the definition given a few lines earlier on p. 92 of the DOE CWG report that states: “CIDs are those features of the weather and climate system of primary interest in assessing the impacts of climate change since they potentially affect humans and the natural world”. Given the proven very large role of increased atmospheric carbon dioxide levels on the global mean temperature and many other aspects of the climate system, it would certainly meet the definition of a CID.

Suggested action: Remove the line suggesting that increase in CO_2 levels does not constitute a CID.

Comment: Technical

Page 93 includes the following lines “Of the five high confidence assertions, two are for changes in average temperatures (air and ocean) hence are not measures of extreme weather. Further, two of the four medium confidence assertions are related to ocean chemistry and thus are likewise not related to extreme weather. The IPCC does not assert a human influence on many non-temperature weather features such as wind, precipitation, flooding, or drought.”

These statements confirm the misunderstanding of what constitutes a Climate Impact-Driver (CID). As discussed above, CID are not just trends.

This section also repeats the misunderstanding what the white cells in the table represent. In may cases, the lack of emergence of anthropogenic influence in many CIDs is due to a lack of studies rather than a lack of a signal. As noted previously, the IPCC report states on p. 1855 “Studies are missing to properly assess S/N emergence for droughts and for wind CIDs.”

Suggested action:

- Edit the text to reflect that CIDs are not isolated to climate extremes.
- Acknowledge that a lack of peer-reviewed literature challenges the robust detection of anthropogenic signals in the emergence of climate changes.

Comment: Technical

There follows a discussion of issues with RCP8.5. This follows other text on this scenario on Pages 15 and 16 in the DOE CWG report. A response to section 3.2 has also been provided.

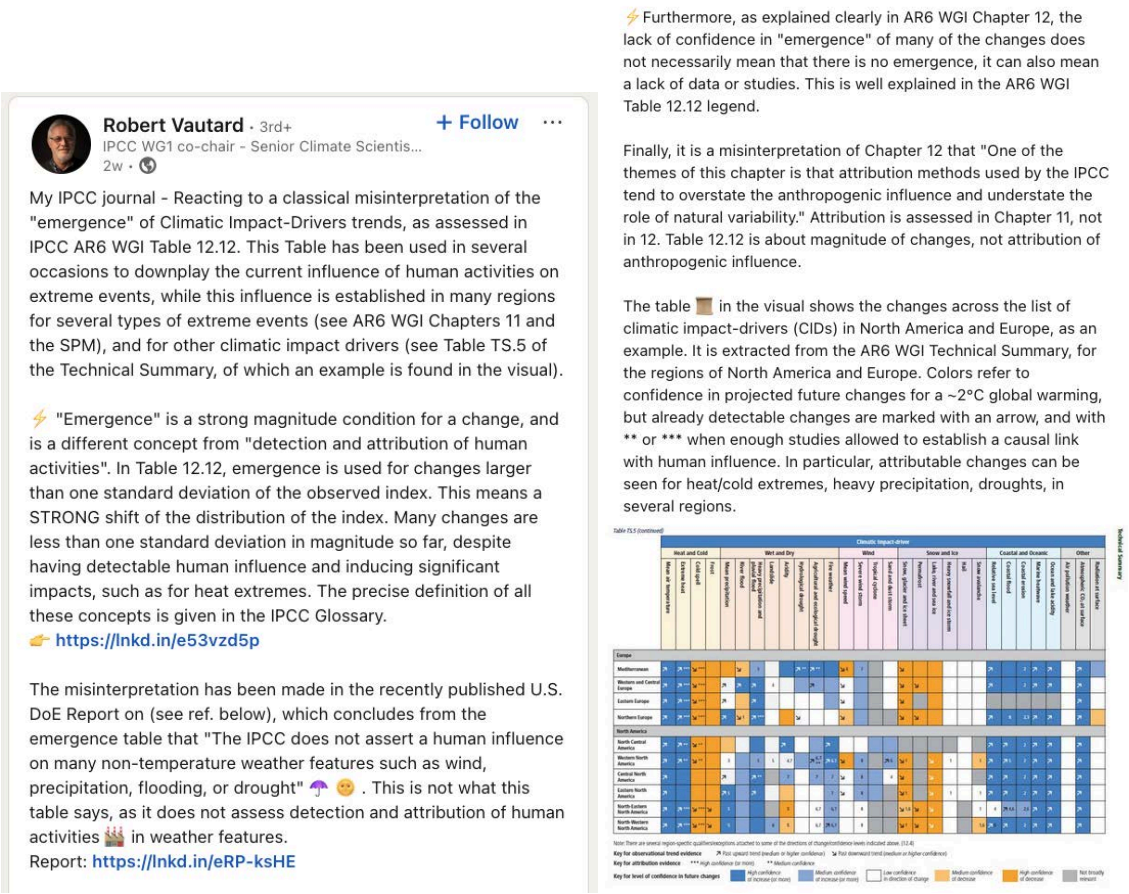
Suggested action: Edit this section to reflect the comments raised in response to section 3.2.

Comment: Technical

Page 93 of the DOE CWG report goes on to state “As discussed in Chapter 6 natural variability dominates patterns of extreme weather systems and simplistic assertions of trend detection are frequently undermined by regional heterogeneity and trend reversals over time. Table 8.1 makes the related point that it is not currently possible to attribute changes in most extreme weather types to human influences. Taking wind as an example, the IPCC claims that an anthropogenic signal has not emerged in average wind speeds, severe windstorms, tropical cyclones or sand and dust storms, nor is one expected to emerge this century even under an extreme emissions scenario. The same applies to drought and fire weather.”

Again, this confuses CIDs with simple trends. There are many places in the IPCC report (e.g., chapter 11) where trends in various quantities have been identified, although there might not be a trend in a CID.

Suggested action: Edit this section to better reflect the definition of a CID and clearly acknowledge that the white cells in Table 8.1 (12.12 in the IPCC AR6 WG1 report) refer to a lack of available literature as well as a lack of detectable signal.



Robert Vautard · 3rd+
IPCC WG1 co-chair - Senior Climate Scientis...
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My IPCC journal - Reacting to a classical misinterpretation of the "emergence" of Climatic Impact-Drivers trends, as assessed in IPCC AR6 WGI Table 12.12. This Table has been used in several occasions to downplay the current influence of human activities on extreme events, while this influence is established in many regions for several types of extreme events (see AR6 WGI Chapters 11 and the SPM), and for other climatic impact drivers (see Table TS.5 of the Technical Summary, of which an example is found in the visual).

⚡ "Emergence" is a strong magnitude condition for a change, and is a different concept from "detection and attribution of human activities". In Table 12.12, emergence is used for changes larger than one standard deviation of the observed index. This means a STRONG shift of the distribution of the index. Many changes are less than one standard deviation in magnitude so far, despite having detectable human influence and inducing significant impacts, such as for heat extremes. The precise definition of all these concepts is given in the IPCC Glossary.
👉 https://lnkd.in/e53vzd5p

The misinterpretation has been made in the recently published U.S. DoE Report on (see ref. below), which concludes from the emergence table that "The IPCC does not assert a human influence on many non-temperature weather features such as wind, precipitation, flooding, or drought" ☂️ 😶 . This is not what this table says, as it does not assess detection and attribution of human activities 🎂 in weather features.
Report: https://lnkd.in/eRP-ksHE

⚡Furthermore, as explained clearly in AR6 WGI Chapter 12, the lack of confidence in "emergence" of many of the changes does not necessarily mean that there is no emergence, it can also mean a lack of data or studies. This is well explained in the AR6 WGI Table 12.12 legend.

Finally, it is a misinterpretation of Chapter 12 that "One of the themes of this chapter is that attribution methods used by the IPCC tend to overstate the anthropogenic influence and understate the role of natural variability." Attribution is assessed in Chapter 11, not in 12. Table 12.12 is about magnitude of changes, not attribution of anthropogenic influence.

The table 🟫 in the visual shows the changes across the list of climatic impact-drivers (CIDs) in North America and Europe, as an example. It is extracted from the AR6 WGI Technical Summary, for the regions of North America and Europe. Colors refer to confidence in projected future changes for a ~2°C global warming, but already detectable changes are marked with an arrow, and with ** or *** when enough studies allowed to establish a causal link with human influence. In particular, attributable changes can be seen for heat/cold extremes, heavy precipitation, droughts, in several regions.

Figure 1. LinkedIn post from IPCC chapter 12 coordinating lead author Robert Vautard discussing how the DOE CWG report does not interpret climate impact-drivers correctly.

Section 8.6 Extreme event attribution (EEA)

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Synopsis

This section of the DOE CWG report, on pages 95-96, attempts to assess the evidence for the anthropogenic influence on extreme weather and climate events through the lens of extreme event attribution. It first misrepresents the findings of the IPCC. It then focuses on the work of a single group (World Weather Attribution) using only one type of EEA method, neglecting the rest of the field. It provides almost no citations to relevant peer-reviewed literature in the field of EEA.

To correct these issues, the DOE CWG report needs to:

- Accurately assess the field of EEA, which constitutes hundreds of peer-reviewed studies using a range of different methods, all with different benefits and limitations. This requires accurately citing relevant peer-reviewed scientific literature.
- Provide citations to relevant peer-reviewed scientific literature when critiquing methods, both to describe those methods clearly and to support relevant criticisms
- Accurately reflect the underlying question of EEA, which does not claim or seek to show that anthropogenic warming ‘causes’ any given event, but that it may influence the properties (likelihood, magnitude, duration) of any given event or class of events.

More detailed comments are provided below. In several cases, comments are of the form ‘Technical/Reference’, where both technical flaws are found and the relevant literature is either missing or is misinterpreted.

Detailed Assessment of Report Conclusions

Comment 1

Page: 95
Comment type: Technical/Reference
Comment:
The DOE CWG Report attempts to contrast the conclusions of Chapters 11 and 12 of the IPCC AR6 Working Group 1 (WG1) report, claiming that the report as a whole provides an "ambiguous assessment of the role of anthropogenic warming" [page 95]. This argument is a misleading interpretation of the IPCC report. The conclusions for these two chapters are consistent because they address different questions. To make this misleading comparison, the authors of the DOE CWG Report make inaccurate statements on the results from Chapter 12. These comments are explained below.

The climatic-impact driver (CID) analysis in WG1 Chapter 12 focuses on the *emergence* of a climate change signal, in either means or extremes (Ranasinghe et al., 2021). An emergence of a climate change signal "occurs when that signal exceeds some critical threshold (usually taken to be a measure of natural variability; see for example Hawkins and Sutton, 2012) or when the probability distribution of an indicator becomes significantly different to that over a reference period (e.g., Chadwick et al., 2019)" (Section 12.5.2, Ranasinghe et al., 2021). In Table 12.12 specifically, emergence refers to changes larger than one standard deviation of the observed index. It does not assess detection and attribution of anthropogenic forcing in extreme weather occurrence. This is also discussed in the response to Section 8.5 by King and Dessler.

WG1 Chapter 11 instead focuses on *trend and event attribution* of extreme weather and climate events (Seneviratne et al., 2021). Trend attribution, documented by the IPCC since AR4 (Hegerl et al., 2007), explains the causes of long-term trends in mean climate conditions, and also in certain extreme weather or climate indices. Aggregating across regions helps to identify changes that are influenced by global scale drivers such as greenhouse gas increases and allows careful separation of the influence from different drivers such as solar forcing and aerosols. Many changes have detectable human influences, but are less than one standard deviation in magnitude so far. The authors of the DOE CWG report can refer to the IPCC Glossary for definitions of each concept (https://apps.ipcc.ch/glossary/).

A further issue arises in the interpretation of Table 12.12. Here, confidence in emergence is conflated with data availability, meaning that a white cell ("low confidence in direction of change") could indicate either that there is no emergence, or that there is a lack of data or studies (Table 12.12 caption, Ranasinghe et al., 2021; see comments on section 8.5 of this report).

Comment 2

Page: 95
Comment type: Technical
Comment:
This section of the DOE CWG report focuses solely on the work of World Weather Attribution (WWA). This is a significant misrepresentation of the field of extreme event attribution (EEA). WWA primarily uses probabilistic event attribution, which is only one of several EEA methods, and is only one of many groups that perform EEA studies. Beyond probabilistic attribution, there are many other groups and institutions using and developing different EEA methods, which are not discussed in this report at all. These methods include, but are not limited to, forecast-based attribution (e.g. Leach et al., 2024; Ermis et al., 2024; Reed et al., 2020), spectrally nudged storylines (e.g. Sanchez-Benitez et al., 2022; Klimiuk et al., 2025), and pseudo-global warming (e.g. Lenderink et al., 2025; de Vries et al., 2024). This omission greatly limits the scope of this report in its review of EEA.

In addition, the description of the probabilistic WWA approach is also misleading and implies that it uses a single line of evidence (regional climate models) rather than a well-established and peer-reviewed method combining multiple lines of evidence (Philip et al., 2020a, van Oldenborgh et al., 2021). The WWA approach uses multiple sources of observational and reanalysis data, as well as a combination of global and regional climate models relevant to the event of interest. Furthermore, in the case of complex or multivariate events, multiple methods may be used to provide additional insight (e.g. Barnes et al., 2025; Clarke et al., 2024). Other organizations conduct EEA analyses using this approach, including several national meteorological services, such as the Australian Bureau of Meteorology (e.g. Hope et al., 2024), the Royal Netherlands Meteorological Institute (e.g. Philip et al., 2022; Philip et al., 2020b), the UK Met Office (e.g. Christidis et al., 2023), the German meteorological service (e.g. Schröter et al., 2024), the Japan Meteorological Agency (e.g. Kawase et al., 2022), and the Meteorological Service of Canada (e.g. Gillett et al., 2022).

Comment 3

Page: 95
Comment type: Technical/Reference
Comment:
The DOE CWG report claims that “WWA’s extensive promotion of non-peer-reviewed findings, its open admission to shaping analyses to serve litigation, and its methodological challenges have sparked controversies, with critics questioning the robustness and impartiality of their conclusions (Pielke Jr. 2024)”.

WWA studies use rigorous peer-reviewed methods (Philip et al., 2020a, van Oldenborgh et al., 2021; Ciavarella et al., 2021; Otto et al., 2024). Furthermore, many studies have been peer-reviewed in the aftermath of the initial rapid study (e.g. van Oldenborgh et al. 2017; Zachariah et al., 2022; Kimutai et al., 2025). To date, 26 such studies have been peer-reviewed,

with no significant change to the results or conclusions. In making this point, the DOE CWG report implies that the peer review process is important, especially for a document used to inform government policy. The scientific community agrees strongly with this sentiment.

The claim that WWA has an "open admission to shaping analyses to serve litigation" is unfounded and false. The purpose of WWA as set out publicly on its website (https://www.worldweatherattribution.org/about/, accessed 08/07/2025) is as follows:

*"Working with scientists around the world, WWA quantifies how climate change influences the **intensity** and **likelihood** of an extreme weather event in the immediate aftermath of the extreme event using weather observations and computer modelling. To encourage actions that will make communities and countries more resilient to future extreme weather events, WWA studies also evaluate how existing vulnerability worsened the impacts of the extreme weather event. The results are made public as soon as they are available, often days or weeks after the event, to inform discussions about climate change and extreme weather."*

Finally, the "controversies" and "critics" all refer to a single reference of a blog article by a single author "(Pielke Jr. 2024)", rather than robust peer-reviewed scientific literature. There are also no citations to the abundant peer-reviewed EEA literature itself in making these claims.

Comment 4

Page: 95
Comment type: Technical/Reference
Comment:
In reference to the WWA method, the DOE CWG report further claims that "Technical criticisms of the approach include a lack of a formal detection process; an implicit assumption that 100 percent of the post-industrial warming is caused by greenhouse gases; and a failure to adequately account for internal climate variability." Given the lack of citations behind these claims, they are simply taken exactly as written.

First, it is unclear what is meant by the "lack of a formal detection process", and as discussed above, the process is detailed and published with peer-reviewed papers published in scientific journals linked to the WWA website.

Second, on the "implicit assumption that 100 percent of the post-industrial warming is driven by greenhouse gases," IPCC WG1 Chapter 3 concluded that approximately 100% of global surface temperature change from 1850-1900 to 2010-2019 was anthropogenically driven (Eyring et al., 2021), and the DOE CWG report does not provide evidence against this conclusion. Furthermore, model analysis does not assume 100% of warming is anthropogenically driven; instead, comparing the historical and early parts of future simulations with naturally forced or pre-industrial control simulations makes no underlying assumption about the cause of warming.

Third, a "failure to adequately account for internal climate variability" is unclear, because "adequately" is not a scientifically defined term. This is compounded by the lack of supporting

citation. However, this is also an outdated criticism more broadly. The peer-reviewed WWA method has been updated (also via peer review) to account for internal climate variability, by including relevant mechanisms (such as the El Nino Southern Oscillation or Indian Ocean Dipole) as additional covariates in the statistical modelling. These are included in any EEA study in which these factors are known to influence extremes in the region (e.g. Kimutai et al., 2025).

Comment 5

Page: 95
Comment type: Technical/Reference
Comment:
The DOE CWG report raises the challenge of the "lack of data" for EEA. There are a range of established methods for dealing with this issue. In the case of probabilistic attribution, which the report focuses on solely, the main approach is to use many data sources.

First, several observational and reanalysis datasets exist for EEA analysis. These do often begin in 1950 (e.g. ERA5; Hersbach et al., 2020) or in the 1970/80s, but may go back to 1920 or earlier (such as E-OBS; Cornes et al., 2018, and CRU TS; Harris et al., 2020), depending on the geographical region and temporal resolution. As the authors identified in previous sections (e.g. section 6.4, p. 61), weather station records often go back more than a century, and the very longest composite records go back even further. For instance, the central England temperature record stretches back to the 1600s (Legg et al. 2024).

The second source of data is climate models, which is not mentioned in the DOE CWG report. This includes but is not limited to global and regional climate models. By using many such models, each of which contributes approximately a century of data, any given probabilistic attribution study draws on several hundred to several thousand years of data. This is a basic, foundational and widely understood aspect of probabilistic event attribution, backed by peer-reviewed literature.

More broadly, the report claims that the aim of EEA is only to identify and study events that are beyond "thresholds of what is expected from natural variability". On the contrary, it is very often the case that a dangerous extreme weather event would have been possible with or without the influence of human induced climate change. However, the likelihood of an event exceeding a given magnitude (or, equivalently, the magnitude of an event of a given likelihood) may have changed because of human induced climate change.

Comment 6

Page: 96
Comment type: Technical
Comment:

This section of the DOE CWG report also highlights that assessing changes in different extreme atmospheric events with similar meteorological impact variables is a challenge. However, as in the rest of the section, it oversimplifies the approaches taken in probabilistic attribution studies to address this challenge. In these studies, the degree to which the statistical model fits the data is then investigated. This enables a judgment over whether the extremes in question can be well represented by a single distribution to enable an EEA study. The climate models used are also assessed for their ability to represent such extremes.

More importantly, the DOE CWG report neglects all other EEA methods. Almost all other EEA methods are more highly 'conditioned' than the probabilistic approach. This means that they begin the attribution analysis by conditioning on the atmospheric state, or circulation "regime", such as the inland blocking event resulting in a heatwave as described in the DOE CWG report (Shepherd, 2016; Trenberth et al., 2015). The weather event resulting from this unusual atmospheric state can then be perturbed using well understood thermodynamic influences of climate change, such as observed and projected changes in sea surface temperatures. The exact details vary by method (see comment on the range of EEA methods and references therein), but broadly this enables an assessment of how such an event in the same "regime" unfolds in warmer and cooler climates. This challenge has been well understood and addressed by the EEA community for nearly a decade. The lack of references to any of this large body of work again illustrates the significantly limited scope of the DOE CWG report.

Comment 7

Page: 96
Comment type: Technical/Reference
Comment:
The report has misinterpreted the overarching message of the literature on EEA. On page 96, the DOE report states "Visser and Petersen argue that, in view of the deep uncertainties of extreme weather analysis, drawing a connection between individual events and global climate change should be avoided." However, Visser and Petersen (2012) explicitly agree with a previous study (Schär and Jendritzky 2004) who stated the following: "The European heatwave of 2003: was it merely a rare meteorological event or a first glimpse of climate change to come? Probably both.", as well as with Stott et al. (2004), a foundational paper of EEA, which states: "It is an ill-posed question whether the 2003 heatwave was caused, in a simple deterministic sense, by a modification of the external influence on climate – for example, increasing concentration of greenhouse gases in the atmosphere – because almost any such weather event might have occurred by chance in an unmodified climate."

This reflects the wider view of the attribution community: no extreme weather events are caused by climate change but all extreme weather events may be influenced by it. This was firmly established in the EEA literature in 2012, when two apparently contradictory studies were reconciled by Otto et al. (2012), who showed that an event could both be caused predominantly by natural variability and influenced by climate change. In conclusion, the literature does not support the claim in the DOE CWG report that "drawing a connection between individual events

and global climate change should be avoided". This point is developed in the following case study.

Section 8.6.1: Case study – 2021 Western North America heat wave

This section of the DOE CWG report, on pages 96-97, summarizes the evidence on anthropogenic influence on the June 2021 Pacific Northwest (PNW) extreme heat event. They use this event as a "case study" to "illustrate the challenges and ambiguities in attributing the frequency and intensity of extreme weather events to human-caused warming." As detailed below, the DOE CWG report accurately summarizes the meteorological conditions that contributed to the heat event. However, they reach misleading conclusions regarding the overall human influence on this event.

To correct these issues, the DOE CWG report needs to:

- Distinguish between meteorological drivers of extreme events and the thermodynamic intensification of those events due to climate change.
- Accurately reflect the consensus of studies regarding the role of warming in producing warmer surface temperatures during the 2021 PNW heat wave.

Comment: Editorial

The DOE CWG report contrasts the WWA attribution study of this event with "subsequent peer-reviewed analyses" showing that this heat wave "was caused by rare meteorological conditions that were not made more probable by global warming" (p. 97).

This statement is misleading. Extreme weather events in a warming world occur due to the *combination* of anomalous meteorological conditions *and* the influence of climate change (Trenberth et al., 2015). In the case of the PNW, the meteorological conditions were indeed extreme, resulting in a surface heat wave that would have been high-impact regardless of the background climate. However, the scientific evidence is clear that global warming thermodynamically increased the temperature of the PNW heat wave, even if it did not affect the intensity of the high-pressure system or other meteorological precursors. This idea is reflected in numerous peer-reviewed studies of this event:

- McKinnon and Simpson (2022) state that "The meteorological causal factors for the heatwave occurred on top of a changing mean state due to human influence on the climate system."
- Bercos-Hickey et al. (2022) conclude that "there is little doubt that anthropogenic global warming contributed to the probability and magnitude of the extreme temperatures."
- Bartusek et al. (2022) find that "even gradual warming over recent decades has dramatically transformed the character of this extreme heat event," and that "[c]ontinued warming will cause the probability of an equal or stronger event to rapidly increase."

Comment: Reference

While the DOE CWG report cites many relevant and high-quality references in this section, they appear to **cherry-pick sentences to downplay the influence of climate change on the event**.

For example, the report cites the 2023 Oregon Climate Assessment (Fleishman, 2023) to state that climate change did not make the "heat dome" more likely. Yet the report neglects the explicit statement of the Oregon Climate Assessment that "[a] heat dome would have occurred without climate change, but **maximum temperatures would not have been as high**".

Additionally, the DOE CWG report cite McKinnon and Simpson (2022) to state that the event was "bad luck," but ignore the paper's statements that "[c]hanges in the mean state alone will increase the probability, intensity, and duration of heat waves (Meehl & Tebaldi, 2004); this shift is a well-understood consequence of climate change," and that "climate change added additional warming to the picture." Indeed, the citation of McKinnon and Simpson (2022) illustrates why care is needed in faithfully representing the work of scientists in this area: this paper is particularly interested in understanding "whether the probability of very extreme events is changing *faster than would be predicted by a change in the mean*" (emphasis added). Therefore, they *remove* the forced signal of climate change from their climate model simulations before analyzing the probability of the PNW heat wave. The language of "bad luck" therefore refers to the occurrence of very extreme temperatures on top of a mean shift linked to climate change. This nuance is not considered in the DOE CWG report, which appears more concerned with cherry-picking particular phrases that allow them to dismiss the role of climate change.

Comment: Editorial

Much of this section of the DOE report is concerned with the difficulties of confidently estimating the probability of very extreme events, given that they are by nature very rare (p. 97). This statement is correct, but **several methodological developments not discussed by the report enable more confident attribution of extremely rare events.** Zhang et al. (2024) leverage the spatial dependence of weather stations in the PNW, enabling them to better represent the PNW event in extreme value statistics and leading to the conclusion that "anthropogenic GHG emissions increased the likelihood of observed extreme temperatures in 2021." Alternative approaches include "storyline" attribution, where a regional climate model is run under both a realistic scenario and a "counterfactual" scenario without global warming. This approach does not require estimating uncertain extreme value distributions, and allowed Bercos-Hickey et al. (2022) to conclude that "the anthropogenic temperature increase is about 1 °C."

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Response to Department of Energy Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate, July 2025 - Chapter 9: Climate Change and US Agriculture

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Below are technical comments to statements made within Chapter 9 of the DOE CWG Report. The page numbers associated with each statement/comment grouping indicates the page on which the statement starts.

Overview of Comments on this Chapter

Page 104-108

Comment Type: Technical

This chapter presents an incomplete and misleading picture of the relationship between rising atmospheric CO_2 and agricultural productivity. It focuses narrowly on the direct fertilization effect of CO_2—its potential to boost crop yields under ideal conditions—while largely ignoring the broader and well-documented negative impacts of CO_2-driven climate change on crop yields. The scientific consensus is that CO_2-induced climate change, including rising temperatures, shifting weather patterns, and altered precipitation, will outweigh the benefits of CO_2 fertilization, leading to net yield declines across major agricultural regions in the U.S. and globally (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Muehe et al., 2019; Zhu et al., 2023).

The chapter acknowledges that elevated CO_2 levels can dilute the nutrient content of crops, but downplays the significance of this issue for human health by overstating the potential of biofortification and dietary supplementation to fully offset nutrient deficiencies. The chapter fails to acknowledge a growing body of literature that links CO_2-induced climate change, including rising temperatures, to increased concentrations of toxins in crops.

ACTION: The chapter needs to accurately reflect the full scope of what is known about the impacts of CO2 and CO2-induced climate change on agriculture, including the likelihood of yield reductions and food quality risks, rather than focusing narrowly on CO_2 fertilization effects

Chapter summary

Page 104

Comment Type: Technical

DOE CWG STATEMENT: "*There has been abundant evidence going back decades … that CO2-induced warming will be a net benefit to U.S. agriculture."*

COMMENT: The DOE report provides little evidence that CO2-induced warming will be a net benefit to U.S. agriculture and ignores a vast literature which reaches the opposite conclusion. The only section in which temperature is discussed as having a potentially positive impact on agriculture is Section 9.1. (Note: Section 9.3 incorrectly claims that Figure 9.2 shows a yield increase with temperature; see response to section 9.3 below). Section 9.1 reviews economic studies that use historic data to connect climate variations with agricultural land values under the assumption that "if climate change is a long-term net benefit for agriculture it should be capitalized into higher market values for agricultural land." The reviewed studies had a range of outcomes. Three found warming led to declines that were statistically significant, one found warming had no perceivable impact, two found warming led to a slight benefit, and one study found warming led to a large benefit for French agriculture, which is not expected to be relevant to the U.S. context. This range in responses is far from supporting the statement that warming will be a net benefit to U.S. agriculture.

Indeed, DOE CWG Section 9.1 completely ignores a vast literature spanning methodological approaches that reaches precisely the opposite conclusion, finding large and robust agricultural losses from CO_2-induced warming both for the U.S. and for the globe, several of which already account for gains from CO_2 fertilization (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hu et al., 2024; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Su et al., 2023; Zhao et al., 2017). Further, physiological studies of crops show there is an optimal temperature at which yields maximize. When temperatures are below or above this optimal point, yields decline (Djanaguiraman et al., 2020; Khan et al., 2021; Neild & Newman, 1990; Su et al., 2023). Therefore, positive historic responses to temperature could reflect plants reaching their optimal growing conditions. Pushing temperature beyond this point will not result in a continued increase, but rather a yield decrease.

ACTION: The authors need to remove the statement that there has been abundant evidence going back decades that CO_2-induced warming will be a net benefit to U.S. agriculture. This statement is unsupported and does not align with current understanding.

Page 104

Comment Type: Technical

DOE CWG STATEMENT: *"There is reason to conclude that on balance climate change has been and will continue to be neutral or beneficial for most U.S. agriculture."*

COMMENT: Again, DOE CWG Chapter 9 completely ignores a vast literature spanning methodological approaches that finds large and robust agricultural losses from climate change both for the U.S. and for the globe, several of which already account for gains from CO_2 fertilization (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hu et al., 2024; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Su et al., 2023; Zhao et al., 2017). The overwhelming balance of the literature directly contradicts this statement in the DOE CWG Report.

ACTION: The authors need to remove the statement that "on balance climate change… will continue to be neutral or beneficial for most U.S. agriculture" and replace it with a summary that actually reflects the broad literature on this issue – namely that it is expected that CO_2-induced climate change will lead to agricultural losses.

9.1 Econometric analyses

Page 104

Comment Type: Technical

DOE CWG STATEMENT: "*Mendelsohn et al. (1994) examined the relationship between historical climatic variations on agricultural land values and concluded global warming would be slightly beneficial to American agriculture. The Mendelsohn et al. method, called Ricardian analysis after David Ricardo, the 19th century British economist who pioneered the study of land values, attracted subsequent criticism from authors who argued it failed to account for differences in land values attributable to fixed locational characteristics like soil quality, and non-climatic changes including nearby urbanization."*

COMMENT: This report's reliance on the Mendelsohn et al (1994) findings (hereafter "MNS") is a misleading summary of the state of the literature. MNS – published over 30 years ago – employs a cross sectional regression estimator that is now widely recognized as vulnerable to confounding, or "omitted variables bias" as it is termed in the econometrics literature broadly (Angrist & Pischke, 2009, Chapter 3; Wooldridge, 2010, Chapter 4). This is a fundamental problem with the empirical approach employed by MNS and is also widely recognized in the literature on climate change and agriculture (see for example reviews Auffhammer & Schlenker, 2014; Hsiang, 2016). In particular, work following MNS showed that the MNS estimator omitted irrigation, confounding profits in warmer U.S. climates with the irrigation systems present in those climates (Schlenker et al., 2005). Once they accounted for this issue, Schlenker et al. (2005) found losses to farm land values in the Central and Eastern U.S. under future warming.

ACTION: This report should include a discussion of the methodological shortcomings in MNS, including discussion of more recent follow-on work in Schlenker et al 2005 and its critique of the MNS cross sectional hedonic approach. The critique of the cross-sectional hedonic approach is widespread in the literature (Auffhammer & Schlenker, 2014; Burke & Emerick, 2016; Hsiang, 2016; Hultgren et al., 2025; Schlenker & Roberts, 2009)

Page 104

Comment Type: Technical

DOE CWG STATEMENT: "*Deschênes and Greenstone (2007) looked at agricultural profits instead of land values and reached conclusions similar to Mendelsohn et al. (1994), namely that past climatic variations had relatively little effect on farm profitability and that warming would likely yield small overall benefits for the U.S. agricultural sector. However, a subsequent exchange with critics led them (Deschênes and Greenstone 2012) to revise their conclusions and project potentially large losses in U.S. agriculture due to climate warming."*

COMMENT: As is well known in the literature, the original Deschênes and Greenstone (2007) results discussed here were due to a severe data error in which weather data was not correctly matched to U.S. counties (as clearly documented in Fisher et al (2012) Figures 1 and 2). Thus the original Deschênes and Greenstone (2007) results should simply be ignored (though the important methodological contribution can be noted): the Deschênes and Greenstone (2007) results are the product of a widespread data error, not an actual empirical finding. Note: the change in results in Deschênes and Greenstone (2012) was not simply the result of an "exchange with critics" as stated in this report (phrasing which is open to many interpretations). Rather, the corrected findings in Deschênes and Greenstone (2012) are the result of correcting a severe and pervasive – and clearly documented – data error. The current phrasing in this report is misleading at best.

ACTION: The specific results from Deschênes and Greenstone (2007) should not be included in the report due to the well documented data error. The inaccurate "exchange with critics" statement should also be removed and the updated Deschênes and Greenstone (2012) results instead discussed as a correction to severe data errors in the original Deschênes and Greenstone (2007) publication.

Page 104

Comment Type: Technical

DOE CWG STATEMENT: *"Burke and Emerick (2016) looked at temperature variations over 1980 to 2000 and argued that farmers were not as able to adapt to temperature changes as the Ricardian method assumes and that climate change would have large negative impacts on corn and soy yields. Schlenker and Roberts (2009) similarly argued that yield gains to past warming would not carry over to the future and corn and soy yields would sharply decrease this century due to climate change."*

COMMENT: Burke and Emerick (2016) does not "argue" that farmers are not able to adapt to temperature changes: they explicitly *test* whether or not that is the case. They *find* that farmer adaptation is limited. Note: their test could result in a finding that farmers do substantially adapt; that is simply not what the data show. Similarly, Schlenker and Roberts (2009) do not "argue" that yields would sharply decrease under climate change, this is instead an empirical finding in

their work. The report does not discuss anything about the actual findings of these papers, implying that these are philosophical arguments.

An accurate summary of Burke and Emerick 2016 is as follows: “In Burke and Emerick (2016), the authors construct measures of short run heat shocks as well as long-run changes in heat exposure. If farmers adapt to long run changes in heat exposure (as they would under climate change), then yield losses under long-run heat changes should be significantly smaller than yield losses under short run (unexpected) heat shocks. Instead, Burke and Emerick (2016) finds that yield losses under both measures are nearly identical, leading them to conclude that the scope for climate change adaptation to heat extremes by U.S. farmers is limited.”

The empirical test in Schlenker and Roberts (2009) could have found that yields increase under future climate change, or are unaffected by climate change – but does not, in fact the data support the conclusion that U.S. corn, soybean, and cotton yields will decrease under future warming. Note that this result has proved extremely robust (see for example reviews Auffhammer and Schlenker 2014; Hsiang 2016).

ACTION: The report authors need to substantively discuss the findings of Burke and Emerick (2016) and Schlenker and Roberts (2009) and the empirical tests that they employ.

COMMENT: This report incorrectly attributes the finding “yield gains to past warming” to Schlenker and Roberts (2009). Schlenker and Roberts (2009) do not find “yield gains to past warming” as that is not a topic that paper studies. Historical yields have risen, but that is due to technology development (Pardey & Alston, 2020).

ACTION: The statement “*Schlenker and Roberts (2009) similarly argued that yield gains to past warming”* should be removed.

Page 104

Comment Type: Technical

DOE CWG STATEMENT: *“Two recent studies have argued that pessimistic findings such as these are not robust. Ortiz-Bobea (2019) argued that land values aggregate farm and non-farm influences and the latter need to be filtered out. He developed a data set using cash rents for agricultural activity as a measure of land value specifically for farming activity. Whereas the land value model implied future losses under climate warming, the same model estimated using cash rents did not, leading the author to conclude the pessimistic results were due to using an inaccurate measure of the returns to farming activity.”*

COMMENT: There are three main issues with the statement above: criticism of the methodology, the large uncertainty of the results is not acknowledged, and the larger body of work by the same author with opposite conclusions was omitted from this report. (Note that “Ortiz-Bobea (2019)” was actually published in (2020). For consistency with the DOE report, this

comment will refer to “Ortiz-Bobea (2019)”, but the report reference and citation should be corrected.)

Issue 1a: Methodological criticism #1: First, Ortiz-Bobea (2019) employs the same general method (cross sectional regression) as that employed by Mendelsohn et al (1994). Thus, the same general criticisms apply. Specifically, Ortiz-Bobea (2019) employs a cross sectional regression estimator that is widely recognized as vulnerable to confounding, or “omitted variables bias” as it is termed in the econometrics literature broadly (Angrist and Pischke 2008, Chapter 3; Wooldridge 2010 Chapter 4). This is a fundamental problem with the empirical approach employed and is also widely recognized in the literature on climate change and agriculture (see for example reviews Auffhammer and Schlenker 2014; Hsiang 2016).

ACTION: The report should clearly discuss the issue of confounding in the cross-sectional hedonic regression approach issue as a major caveat to the Ortiz-Bobea (2019) findings.

Issue 1b: Methodological criticism #2: The Ortiz-Bobea (2019) includes an important caveat that should be discussed in the report, a caveat which we copy in full here: *“As previously mentioned, a potential caveat of the current implementation is that the survey data targets cropland and pastures participating in the rental market, and not all cropland and pastures. While the data necessary to carefully analyze this selection problem are unavailable, one can speculate about microlevel selection patterns from aggregate data. Specifically, the fact that counties with better soils tend to exhibit a higher share of rented farmland (online supplementary figures S19 and S4) suggests that parcels with better soil quality are more likely to be rented out. This suggests that high-quality land may be overrepresented in each county. As a result, if land of poor quality is more vulnerable to climate change, then this truncation of the sample would bias Ricardian estimates upward.”* Note that the issue mentioned here is one of selection bias into the data, not bias due to omitted variables, and is thus a separate issue from the first substantive issue raised in this response.

ACTION: This issue of selection bias should be discussed in the report as a bias towards finding cash rent gains from climate change when in fact there may be none.

Issue 2: Large uncertainty of the results: The uncertainty in the Ortiz-Bobea (2019) findings is massive (see Ortiz-Bobea (2019) Figure 3a, 3b). For high warming RCP 8.5, rainfed cropland cash rents could decrease by over 50% or increase by over 90% according to the paper’s own results.

ACTION: This uncertainty in the results of Ortiz-Bobea (2019) should be explicitly acknowledged in the report.

Issue 3: Omission of larger body of work by the same authors with opposite conclusions: It is remarkable that the report cites the one paper by Ortiz-Bobea that finds (possible) profitability gains from climate change, while completely ignoring the plethora of papers by the same author that reach the opposite conclusion.

ACTION: If the report wishes to discuss Ortiz-Bobea 2019, equal weight should be given to that author's other work regarding agriculture and climate change. By order of citation counts, the top 5 papers studying climate change and agriculture by Ortiz-Bobea are: Ortiz-Bobea et al 2021, Ortiz-Bobea et al 2019, Gammans et al 2017, Ortiz-Bobea et al 2018, and Yang et al 2024.

Page 104

Comment Type: Technical

DOE CWG STATEMENT: *"Bareille and Chakir (2023) assembled a large data base on farm sale prices in France for properties that sold twice between 1996 and 2019. They could replicate pessimistic results showing negative effects of warming on agricultural land values using conventional econometric modeling. But by taking advantage of the repeat sales data, which provides information on site-specific changes in land prices, they found the results reversed and implied that climate change will be very beneficial for French agriculture. The authors concluded that, taking adaptation into account, a warming climate would yield positive benefits for French agriculture that were between two and 20 times larger than had previously been estimated. On average, with full adaptation, they concluded that climate changes under the medium RCP4.5 scenario could double the value of French farmland by 2100."*

COMMENT: This section of the DOE CWG Report addresses climate change and U.S. agriculture. Bareille and Chakir (2023) studied French agriculture. The gains from climate change estimated by Bareille and Chakir (2023) are notable. However, the authors note these gains are driven by crop switching into vineyards (Table 8) – a very high value crop that France is particularly well suited for.

ACTION: This report should remove the discussion of Bareille and Chakir (2023) because the vast majority of agricultural lands in the U.S. are not well-suited to vineyard planting due to soil constraints, limiting the applicability of Bareille and Chakir (2023) in the U.S. context. If the report authors choose to keep the reference to Bareille and Chakir (2023) then they need to include the fact that the results are specific to soils which support vineyards and not broadly applicable to U.S. contexts.

COMMENT: The statement that Bareille and Chakir (2023) estimated benefits that are "*between two and 20 times larger than had previously been estimated"* is incorrect: the comparison in the paper is to pooled cross sectional estimates provided in that same paper; it is not a comparison to "previously" estimated results. Further, the pooled cross sectional estimates in Bareille and Chakir (2023) (provided for comparison with their preferred estimates) have known omitted variables bias issues discussed by Bareille and Chakir in the paper. The "*between two and 20 times larger"* comparison discussed in the DOE CWG Report is thus accurately understood as a critique of the cross sectional regression approach--that is, a critique of Mendelsohn et al 1994 and Ortiz-Bobea 2019.

ACTION: The report should correct this mistake and remove reference to the “two and 20 times larger” comparison.

Page 105

Comment Type: Technical

DOE CWG STATEMENT: *A major deficiency of all these studies, however, is that they omit the role of* CO_2 *fertilization. Climate change as it relates to this report is caused by GHG emissions, chiefly* CO_2*. The econometric analyses referenced above focus only on temperature and precipitation changes and do not take account of the beneficial growth effect of the additional* CO_2 *that drives them. As explained in Chapter 2,* CO_2 *is a major driver of plant growth, so this omission biases the analysis towards underestimation of the benefits of climate change to agriculture.*

COMMENT: It is true that the studies the report cites do not account for CO_2 fertilization. However, this does not represent a “major deficiency,” as the DOE CWG Report claims, for two reasons. First, recent work not cited in the report does exactly this. Hultgren et al. (2025) estimated yield losses accounting for adaptation, and accounting for CO_2 fertilization, and found that yield losses at global scale and for the U.S. persisted for maize, soybeans, wheat, sorghum, and cassava. This result of global-scale yield loss was true for both moderate (RCP 4.5) and high (RCP 8.5) future emissions. Only rice was protected from future warming at global scale and in the U.S., however rice is not a major U.S. crop (and South and Southeast Asia maintain their comparative advantage in rice production relative to the U.S.). See Hultgren et al 2025 Figure 3, and Supplementary Figures S13 and S14. This analysis was not biased “towards underestimation of the benefits of climate change to agriculture,” and it found that accounting for CO_2 fertilization did not alter the finding that climate change is damaging to U.S. and global staple crop output. Note also that other studies have found less optimistic results for rice than did Hultgren et al 2025 – see for example Muehe et al 2019, Figure 1, which found losses to rice yields under future climate conditions in plant-growth experiments even after accounting for gains from CO_2 fertilization.

Second, the DOE CWG statement implies that including CO_2 fertilization effects will overcome any negative effects of CO_2 driven climate change. As highlighted in comments related to the next section (Section 9.2), the authors of this report stop before they get to the end of the issue. Elevated CO_2 operating alone does increase crop yields, but **only under non-stress conditions (see Ainsworth and Long (2021)).** Increasing temperatures and related climate-driven meteorological changes will certainly lead to higher stress conditions that diminish or erase CO_2-induced yield gains (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Muehe et al., 2019; Zhu et al., 2023). This fact limits the extent to which CO_2 fertilization can mitigate yield losses under future warming (Ainsworth & Long, 2021). The lack of discussion about these issues in this section is glaring.

The current consensus – across a range of methodological approaches – is that CO_2-induced climate change will lead to net yield declines in major agricultural regions in the U.S. and globally (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hu et al., 2024; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Su et al., 2023; Zhao et al., 2017), and in some regions, declines have already occurred in response to extreme temperatures (Brás et al., 2021).

ACTION: The revised report should acknowledge and discuss each of the two points outlined in the comment.

Finally, when referring to "all of these studies", this report discusses a total of seven studies. However, the econometric literature on climate change and U.S. agriculture is vast, and this report ignores nearly all of it. It is understandable that not every study on the topic can be included in this report. However, that does not excuse the report's propensity to ignore most of the relevant literature: literature reviews exist for exactly the purpose of summarizing a vast literature.

ACTION: This report should summarize the discussions of recent reviews on this topic such as Auffhammer and Schlenker (2014); Hsiang (2016); Carleton and Hsiang (2016); and the discussion of the literature in Hultgren et al (2025).

9.2 Field and laboratory studies of CO_2 enrichment

Section 9.2 begins on page 105 and ends on Page 106. This section of the DOE CWG report overstates the benefits that elevated CO_2 will have for crops in the U.S. for reasons described below.

Page: 105

Comment Type: Technical

DOE CWG STATEMENT: "*One of the ways the effect of* CO_2 *on crop growth has been studied is through "free air enrichment experiments" or FACE plots, in which small sources of* CO_2 *are placed in fields surrounding plants and the growth response to elevated* CO_2 *under varying weather conditions are recorded. Ainsworth et al. (2020) summarizes results from about 250 such studies. They found that elevation of* CO_2 *by 200 ppm caused an average 18 percent increase in crop yield in C3 plants. C4 plants exhibited benefits mainly under drought conditions."*

COMMENT: The statement that elevating CO_2 by 200 ppm caused an average 18% increase in C3 crop yields fails to acknowledge the caveat clearly stated in the abstract of Ainsworth and Long (2021) that "elevation of [CO_2] by ca. 200 ppm caused a ca. 18% increase in yield **under non-stress conditions**." (Note: the DOE CWG report incorrectly referred to the Ainsworth and Long (2021) publication as Ainsworth et al. (2020)). When plants had adequate water and

nutrients and were not subjected to warmer temperatures, elevated CO_2 was able to increase yields, on average, by 18%. However, when plants experienced stress in combination with elevated CO_2, the yield increases were suppressed and in some cases erased (see Section 4 in Ainsworth and Long (2021)). As CO_2 concentrations increase, they will not be the only factor that changes in the Earth system. Temperatures will warm and weather patterns will alter, layering stress on top of increased CO_2 concentrations.

It is disingenuous to overlook these co-occurring factors and focus on the isolated impact of CO_2 on crop yields under non-stress conditions. Indeed, drought has become both more severe and frequent due to climate change in the western US, a site of ample crop production, and is projected to be even more so in the future; see response to Section 6.7 for more details and references. Overall, yield declines are expected under future climate conditions. Multiple greenhouse studies have looked at how CO_2 will interact with elevated temperature (e.g., Muehe et al., 2019, and studies cited by Zhu et al. 2023), and numerous process-based and statistical modeling studies have investigated how CO_2 coupled with water stress, vapor pressure deficit, temperature, etc. will affect yields (e.g., Hu et al., 2024; Hsiao et al., 2019; Jägermeyr et al., 2021). Recent work by Hultgren et al. (2025) finds that substantial losses in agricultural yields – both for the U.S. and the globe – are expected in all major food crops except for rice, even after accounting for CO_2 fertilization and farmer adaptation potential.

ACTION: The report needs to acknowledge and discuss the current consensus – developed with a range of methodological approaches – that in the major agricultural growing regions of the U.S. (and in most parts of the world), CO_2-induced climate change will lead to yield declines (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hu et al., 2024; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Su et al., 2023; Zhao et al., 2017)

Page: 105

Comment Type: Technical

DOE CWG STATEMENT: "*In addition to FACE plot experiments there have been thousands of laboratory experiments on the effects of* CO_2 *on all kinds of plant growth. Here we review some of the results on key U.S. agricultural crops*."

COMMENT: The four paragraphs starting with the statement above discuss laboratory experiments, citing from a website co2science.org. This website does not produce peer-reviewed research but rather compiles cherry-picked examples from the literature. Cherry picking only the most positive responses is guaranteed to give an overly optimistic view of crop yield CO_2 enhancement. There is a reason that FACE experiments are the scientific standard - running laboratory experiments in small pots in greenhouses is well known to give a wide variation in results because of large variation in conditions. In fact, the correlation coefficient for key photosynthesis and productivity traits between plants grown in controlled greenhouse conditions and those grown in field settings is relatively low (0.26) (Poorter et al., 2016), with

greenhouse studies tending to result in larger yield responses to elevated CO_2 than FACE experiments (Ainsworth and Long, 2021).

At issue is not whether elevated CO_2 increases plant productivity, but whether the yield boost associated with elevated CO_2 is at a sufficient magnitude to counteract damage from other aspects of climate change, such as yield loss associated with warming. The magnitudes cited in this section are well above the consensus of the rigorous FACE studies, which themselves often represent only non-stress conditions and thus ignore the negative effects of co-occurring increases in temperature and changes in water availability (see comment above). For example, this section in the DOE CWG Report states that soybean, maize, and wheat would increase under +300ppm by 50.9%, 23.7%, and 67.6%. FACE experiments show an average response of 21%, 0%, and 14% for +200ppm, respectively (Ainsworth and Long 2021). Scaling to 300 ppm would give roughly 30%, 0%, and 21% for FACE, which is far less than this section cites for all three crops.

It is also worth noting that the numbers they cite from the non-peer-reviewed co2science.org website are not the same as the numbers currently reported on the website. The actual values given currently on the website for +300ppm for soybean, maize, and wheat are 47.9%, 32.1%, and 38.5%.

ACTION: The report should not rely on a review of the literature that has not been peer reviewed. Since FACE experimental data are available for assessing the impact of elevated CO_2 on crop yields, the authors should rely on yield results from FACE experiments and not solely rely on numbers from greenhouse experiments.

ACTION: The authors need to expand their consideration of how climate change will impact yields to include more than CO_2 operating alone. The current consensus – across a range of methodological approaches – is that CO2-induced climate change will lead to net yield declines in major agricultural regions in the U.S. and globally (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hu et al., 2024; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Su et al., 2023; Zhao et al., 2017), and in some regions, declines have already occurred in response to extreme temperatures (Brás et al. 2021)

Page: 106, Figure 9.1 and section labeled "Further evidence"

Comment Type: Technical

DOE CWG STATEMENT: *"A 2021 report from the U.S. National Bureau of Economic Research (Taylor and Schlenker 2021) used satellite-measured observations of outdoor* CO_2 *levels across the United States, matched to county-level agricultural output data and other economic variables. After controlling for the effects of weather, pollution and technology the authors concluded that* CO_2 *emissions had boosted U.S. crop production since 1940 by 50 to 80 percent, attributing much larger gains than had previously been estimated using FACE experiments. They found that every ppm of increase in* CO_2 *concentration boosts corn yields by 0.5 percent, soybeans by 0.6 percent, and wheat by 0.8 percent."*

COMMENT: The Taylor and Schlenker (2021) manuscript described as the sole citation in the "further evidence" section and replicated in Fig. 9.1 has not yet been peer-reviewed, but is publicly available as a working paper (we note that the text refers to the year of the manuscript as 2021, but the reference list cites the year as 2023). Peer review is an important part of the scientific process as it can address many methodological and other concerns that are likely to arise with this paper. Specifically:
(1) The Taylor and Schlenker estimates of CO_2 response are very noisy, so that the 90% confidence interval nearly includes 0, and certainly includes the responses (of ~0.1% yield increase per ppm) seen in FACE experiments (Ainsworth and Long 2021). The authors rely heavily on the point estimates for their conclusions, but the Taylor and Schlenker results have very wide error bars, meaning that the approach cannot be considered robust enough to challenge the general results from FACE experiments. Some of their specifications that include time trends even include 0% yield increase per ppm in the 90% confidence interval. (Figure 6 in Taylor and Schlenker (2021)).
(2) Taylor and Schlenker use as a measure of CO_2 exposure estimates from the OCO-2 satellite, which provide total column rather than surface CO_2. It is very likely that these column totals underestimate the true amount of variation at the surface, which is what crops experience. As the authors themselves state in the working paper, "If CO_2 disturbances at ground level phase out in altitude, then the variation we observe in the satellite data would be smaller than the ground-level variation, thereby leading to an upward-biased CO_2 fertilization coefficient." This bias has not been adequately dealt with, and is likely to be revised in a subsequent version of the working paper (author communication). Initial updates by Taylor and Schlenker presented at a recent conference indicate their estimated CO_2 fertilization gains will be revised downward substantially upon correcting for this issue. Again, this is a reason to not rely on studies that have not yet passed through peer review.
(3) Taylor and Schlenker do not state in the manuscript that "CO_2 emissions had boosted U.S. crop production since 1940 by 50 to 80 percent," as is claimed in the DOE report
(4) The DOE report omits the fact that the Taylor and Schlenker paper also states "We reiterate that climate change will likely have a negative impact on agriculture in aggregate, especially in regions exposed to extreme heat"
(5) Taylor and Schlenker are attempting to find the signal of CO_2 driven enhancement of yield from extremely small perturbations of CO_2 (on order less than 4ppm), and have not attempted to quantify other environmental factors which may co-vary with these small CO_2 variations such as changes in clouds and solar radiation.

ACTION: The authors of the DOE CWG report should remove all reference to this study as it is not peer-reviewed and has issues that the study authors are actively working to address.

9.3 Crop Modeling Meta-Analyses

Page: 106-107

Comment Type: Technical

COMMENT: Section 9.3 purports to summarize evidence from crop-modeling meta-analysis relevant to understanding “Climate Change and U.S. Agriculture”. However, the section discusses results from a single study (McKitrick, 2025) and how they compare to one set of results from an earlier study (Moore et al., 2017). This approach presents a partial, incomplete, and dated representation of current understanding. The authors omit consideration of many more recent studies and related lines of evidence:

- Moore et al. (2017) used two alternative approaches to develop conclusions regarding the impact of climate change on the global agricultural sector. This section speaks to one of these approaches (modeling based on the Challinor et al. (2014) dataset) but not the other one, which was based on gridded runs of an ensemble of process-based crop models under the Agricultural Model Intercomparison Project (AgMIP, Rosenzweig et al., 2014). Although this second approach produced slightly smaller estimates of crop damage than the meta-analysis, it also supported an overall upward revision in the estimate of climate change impacts on the agricultural sector, relative to those included previously based on estimates from the FUND Integrated Assessment Model.
- Since Challinor et al. (2014) there has been another full assessment round of the IPCC. The relevant chapter in the IPCC 6th Assessment Report (Bezner Kerr et al. 2022) was supported by a much expanded and updated review of the scientific evidence on yield response to changing climate conditions (Hasegawa et al., 2022). This new dataset includes approximately 4 times as many observations as the previous Challinor et al. (2014) dataset. Li et al. (2025) performed a meta-analysis of this dataset using approaches similar to Moore et al. (2017). They find net declines in global yields for major crops, accounting for projected changes in temperature, precipitation, and CO_2, even under relatively low emissions scenarios.
- There are multiple new lines of evidence on the influence of climate change on the global agricultural sector, including a global statistical analysis (Hultgren et al. 2025) and a new round of process-based modeling from AgMIP (Jägermeyr, et al. 2021), which reports that negative climate impacts on crops emerge earlier in the newer generation of models, despite the additional latter finding that these models still underestimate the impacts of extreme events on crop production (Heinicke et al., 2022).

ACTION: This report presents a partial, incomplete, and dated representation of current understanding of evidence of climate impact on agriculture from crop-modeling. The authors should provide a more comprehensive and current assessment of evidence on the effects of climate change on the global agricultural sector, inclusive of references given above.

Page: 106

Comment Type: Technical

DOE CWG STATEMENT: “*Notwithstanding the abundant evidence for the direct benefits of* CO_2 *and of* CO_2*-induced warming on crop growth,...*”

COMMENT: This statement is unsupported by the content of the DOE CWG Report (see response to a similar statement made by the DOE CWG report in the Chapter Summary on pg

104) and it does not align with current understanding. The current consensus – across a range of methodological approaches – is that CO2-induced climate change will lead to net yield declines in major agricultural regions in the U.S. and globally (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hu et al., 2024; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Su et al., 2023; Zhao et al., 2017), and in some regions, declines have already occurred in response to extreme temperatures (Brás et al. 2021)

ACTION: The authors should remove the statement that there is abundant evidence that CO_2-induced warming will be a net benefit to U.S. agriculture.

Page: 106

Comment Type: Technical

DOE CWG STATEMENT: "*in 2023 the U.S. Environmental Protection Agency (EPA 2023) boosted its estimate of the Social Cost of Carbon (SCC) about five-fold based largely on a very pessimistic 2017 estimate of global agricultural damages from climate warming (Moore et al., 2017)*"

COMMENT: This statement that findings from Moore et al. (2017) drove the overall Social Cost of Carbon (SCC) revision estimated by the EPA is unsupported. EPA (2023) introduced many scientific and economic improvements to the calculation of the Social Cost of Carbon (SCC), implementing most of the recommendations from a 2017 National Academies of Sciences report. These improvements include 1) probabilistic projection of socio-economic and emissions trajectories; 2) a much-improved representation of the climate system calibrated to the current ensemble of General Circulation Models (GCMs) and including uncertainty in climate system parameters; 3) updates to damage representation, including three alternative damage modules all using distinct estimation approaches. All these changes interact non-linearly within the SCC calculation, meaning the unique attribution of SCC changes to individual modeling changes, as is done in the referenced statement, is impossible.

The agricultural damage function from the GIVE model, based on Moore et al. (2017), is only one of four damage sectors within GIVE, which in turn is only one of three different damage modules used in the 2023 EPA analysis. Even if, hypothetically, agricultural damages within the GIVE model were completely eliminated, this elimination would result in approximately a $90 per ton change to ⅓ of the damages. A back of the envelope calculation suggests this change would move the 2030 SCC from $230 per ton to $200 per ton (under a 2% discount rate, based on values in Figure 3.1.1 EPA 2023 and Rennert et al. (2022)). Therefore the statement that the 2023 EPA update to the SCC was driven largely by agricultural damages estimates in Moore et al. (2017) is entirely unjustified.

ACTION: The authors should remove this incorrect statement from the report

Page: 107

Comment Type: Reference

DOE CWG STATEMENT: "*McKitrick (2025) re-examined the Moore et al. database*"

COMMENT: The dataset in question was compiled and published by Challinor et al. (2014), used to support analysis in the IPCC 5th Assessment Report Food Security Chapter (Porter et al. 2014). It is inaccurate to refer to it as the Moore et al. dataset

ACTION: Correct all references and discussion within text to refer to the Challinor et al. (2014) dataset.

Page: 107

Comment Type: Technical

DOE CWG STATEMENT: "*McKitrick (2025) re-examined the Moore et al. database and found that, while it claimed to cover 1,722 studies, only half the entries (N=862) had complete records*"

COMMENT: Nowhere in Moore et al. (2017) is it claimed that 1,722 observations were used for the analysis. Moore et al. (2017) report using 1,010 observations, derived from the subset of Challinor et al. (2014) data relevant to the 4 crops studied (rice, wheat, maize and soybeans), where complete data was either present or could be inferred from reported information.

ACTION: Correct the statement to reflect that Moore et al. (2017) report using 1010 observations from the Challinor et al. (2014) database.

Page: 107

Comment Type: Technical

DOE CWG STATEMENT: "*The crop yield projections incorporating the newly available data changed considerably*."

COMMENT: This statement is not supported by the referred to plot (Fig. 9.2). In most cases, particularly for most relevant near-term warming levels of 1, 2, and 3 degrees, uncertainty bounds of estimates from the two studies overlap. The one exception is soybeans, which is a smaller component of both calories and value share in agriculture than wheat, maize or rice.

ACTION: The authors should either remove this statement or perform the appropriate statistical test to determine at what confidence level the two versions of crop response functions (from Moore et al., 2017 and McKitrick, 2025) can be distinguished from each other at each level of warming.

Page: 107

Comment Type: Technical

DOE CWG STATEMENT: "*As shown in Figure 9.2, whereas the partial data set implied warming would decrease yield (blue lines), the complete data set implied constant or increase global yields, even out to 5°C warming (green lines)*."

Figure 9.2 and this description omit relevant information needed to infer the overall response of U.S. Agricultural yields to temperature, as outlined in the following specific comments:

COMMENT: Econometric specifications in both McKitrick (2025) and Moore et al. (2017) include interactions with baseline temperature, meaning estimated warming responses might be different in warm vs cool locations. Figure 9.2 does not identify what baseline temperature is used, nor does it clarify how the responses might look different in other locations.

ACTION: The authors should specify the baseline temperatures used for plotting the response functions in Figure 9.2 and state how the chosen functions relate to temperature conditions for U.S. agriculture, given U.S. agriculture is the topic of the section. Ideally the authors should plot response functions for baseline temperatures typical of U.S. agriculture. The authors also need to describe what confidence intervals are shown in the plotted error bars and clarify whether these response functions are inclusive of CO_2, adaptation, and temperature effects, or show only the temperature response.

COMMENT: Since response functions in both McKittrick (2025) and Moore et al. (2017) allow for heterogenous, non-linear responses across different baseline temperatures, the implications for global yields cannot be inferred from the single set of response functions shown in Figure 9.2, unless these responses have been aggregated up on a production-weighted basis across all global growing regions.

ACTION: The authors should explain how they inferred the aggregate global yield response from the heterogeneous responses estimated in McKittrick (2025). Since Section 9 is on "Climate Change and U.S. Agriculture" they should explain how the U.S. yield response is distinct from the global yield response.

COMMENT: This statement incorrectly interprets the results of McKitrick (2025) presented as the green lines in Figure 9.2 as showing an increase in global yields. In all but one data point (Wheat at 1 ^{o}C change in temperature) the error bars cross the x-axis where yield change is 0%. The correct interpretation of the presented results would be that the complete data set implied only constant global yields, not increasing yields.

ACTION: The authors of the DOE CWG report need to remove "or increase" from the statement such that the sentence correctly states the data set, as presented, only "implied constant global yields."

9.4 CO_2 fertilization and nutrient loss

Section 9.4 begins on page 107 and ends on Page 109. This section of the DOE CWG report minimizes the negative impacts that rising CO_2 levels and associated warming temperatures will have on the nutrient quality of crops to conclude that rising CO_2 levels and CO_2-induced warming will be a net benefit to U.S. agriculture.

As detailed below, this section misinterpreted many of the cited publications — including a publication (Ebi et al., 2021) that we and others co-authored — and failed to consider literature showing that elevated CO_2 and associated warming can increase toxin levels in crops, not just decrease nutrient density. Point-by-point responses are provided to statements in the DOE CWG report in this section.

Page: 107

Comment Type: Technical

DOE CWG STATEMENT: "*Evidence has shown that CO_2 -induced biomass gains are sometimes accompanied by reductions in the concentrations of protein and other key nutrients such as iron and zinc (Ebi et al. 2021).*"

COMMENT: This statement conflates evidence from two specific groups of plants, one of which consistently sees reductions in protein and nutrients under CO_2 and the other of which rarely shows response, to make the results sound less consistent. All plants absorb carbon dioxide from the atmosphere, using photosynthetic pathways to break down that carbon dioxide into carbon and oxygen, and then using the carbon to grow. Eighty-five percent of plants use a photosynthetic pathway that, based on biochemical characteristics, is termed C3; these C3 plants include important crops such as wheat, rice, barley, oats, rye, soybean, and potatoes. Field experiments with wheat and rice under a doubling of CO_2 from preindustrial concentrations demonstrate that protein declines by about 10%, B-vitamins by as much as 30%, and micronutrients by about 5% (see Ebi et al. 2021 and associated references). Wheat and rice each provide about 20% of calories consumed worldwide, with wide regional variability (Awika, 2011; Shiferaw et al., 2013). Corn, sorghum, and millet use a different photosynthetic pathway (C4) and are not expected to experience declines in nutrient density with higher CO_2 concentrations. The DOE CWG report conflates these two photosynthetic pathways into "sometimes" without being clear that declines in nutrient density are consistently seen under elevated CO_2 for C3 crops (Loladze, 2002, 2014; Loladze et al., 2019; Myers et al., 2014; Taub et al., 2008; J. Wang et al., 2019, 2020; Zhu et al., 2018), which are widely consumed in the USA.

The DOE CWG report also ignores a key fact raised in Ebi et al. (2021) that elevated CO_2 and elevated temperature can increase toxins such as arsenic and cadmium in crops (Dhar et al., 2020; Farhat et al., 2021, 2023; Guo et al., 2011; Muehe et al., 2019; J. Wang et al., 2019; Y. Wang et al., 2023; Yuan et al., 2021), not just dilute nutrients. Ingestion of toxins in food results in illness, shortened life span, reduced quality of life, and death (Gibb et al., 2019).

ACTION: The report authors need to remove the word ‘sometimes’ from this statement and modify its wording to clarify the fact that declines in nutrient density are consistently observed under elevated CO_2 for C3 crops but rarely for C4 crops. In addition, the authors need to add a sentence (or section) that acknowledges the fact that research thus far indicates that elevated CO_2 and elevated temperature can increase toxin concentrations in crops.

Page: 107

Comment Type: Technical

DOE CWG STATEMENT: “*Some experiments have shown that the rising temperatures expected to accompany higher CO_2 levels will offset this loss (Köhler et al. 2019) although the evidence for this is mixed, as is the evidence that nutrient dilution observed to date is entirely attributable to higher CO_2 (Ziska 2022).*”

COMMENT: Some experiments have shown that rising temperatures expected to accompany higher CO_2 levels can offset nutritional losses (Köhler et al., 2019), and, while the DOE CWG report acknowledges that evidence for this temperature compensation is mixed, it fails to cite studies that found partial or no compensation in nutritional density when elevated CO_2 was crossed with elevated temperature (e.g., Jayawardena et al., 2021; J. Wang et al., 2019; Wei et al., 2021; Ziska et al., 1997).

ACTION: The report authors need to cite the studies that found partial or no compensation in nutritional density when elevated CO_2 was crossed with elevated temperature.

COMMENT: On the flip side of crop nutritional quality, increased temperatures are expected to increase toxin levels in crops. Wang et al. (2020) found that while warmer temperatures moderately mitigated the loss of nutrients associated with elevated CO_2, the warmer temperatures simultaneously increased Mn, Mo, Cr, Ni, Cd and Pb concentrations in rice and wheat. A study investigating arsenic contamination of rice found that grain arsenic concentrations were more elevated when elevated CO_2 was combined with elevated temperature than when either elevated CO_2 or elevated temperature operated alone (Muehe et al., 2019).

ACTION: The report authors should add a sentence (or section) acknowledging that increased temperatures are expected to increase toxin levels in crops. This sentence or section should cite Wang et al. (2020) and Muehe et al. (2019), at a minimum.

COMMENT: The second half of the statement above (i.e., “… as is the evidence that nutrient dilution observed to date is entirely attributable to higher CO_2”) draws an incorrect conclusion from Ziska (2022). Ziska (2022) presents a figure (Figure 1) that shows newer breeding lines of spring wheat have less percent protein, but that all the breeding lines have experienced declines in percent protein over the 20th century. Thus, Ziska (2022) acknowledges that breeding can change nutritional density but ultimately concludes “[CO_2] is directly affecting protein concentration separate from breeding history.”

ACTION: The report authors need to provide context for the statement that observed nutrient dilution is not entirely attributable to higher CO_2, acknowledging that actions like breeding can

alter nutrient density, while clarifying that elevated CO_2 will operate on top of these other factors, additionally reducing nutrient density.

Page: 107

Comment Type: Technical

DOE CWG STATEMENT: "*If nutrient dilution does occur under rising CO_2 levels, there are several adaptive strategies that could be pursued. First, selective breeding to raise micronutrient content is already established (Saltzman et al. 2017) and has proven to be a cost-effective agronomic strategy (Ebi et al. 2021)*."

COMMENT: Biofortification via selective breeding has been used successfully to raise the zinc, iron, and vitamin A content of crops, per Saltzman et al. (2017). However, this approach is not a 'proven' agronomic strategy, cost-effective or otherwise, for addressing simultaneous nutrient dilution by elevated CO_2. Ebi et al. (2021) specifically stated, "An additional challenge for biofortification is the tendency of rising CO_2 concentrations to diminish the concentrations of multiple nutrients concomitantly (Loladze 2014a, 2014b) in contrast to biofortification that targets only one or a few selected nutrients."

ACTION: The report authors need to clarify that selective breeding to raise micronutrient content is only established for a few selected nutrients and is not yet a proven strategy for addressing the simultaneous decrease of multiple nutrients triggered by elevated CO_2.

COMMENT: Further, biofortification cannot address the issue of increased toxin levels in crops associated with elevated CO_2 and CO_2-induced warming.

ACTION: The report authors need to add a sentence (or section) acknowledging that biofortification will not solve the issue of increased toxin concentrations in crops that is expected to occur with elevated CO_2 and CO_2-induced warming.

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Comment Type: Technical

DOE CWG STATEMENT: "Optimal strategies will be location-specific because they vary by crop, climate and soil type (Ebi et al. 2021)."

COMMENT: Developing new cultivars requires significant investments and generally take years (see CGIAR programs, for example, https://www.cgiar.org). Such programs will not be developed locally, nor will the investment make sense if the developed cultivar only applies to a specific location.

ACTION: The report authors need to add the comment above as a caveat. Additionally, the reference to Ebi et al. (2021) should be removed from this statement because it does not reflect any argument or point made within Ebi et al. (2021).

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Comment Type: Technical

DOE CWG STATEMENT: "*Second, fortification of food products with micronutrients is already routine. Folic acid (a B vitamin) is added to flour and many other foods; iodine is added to table salt, most commercial breakfast cereals are fortified with iron and numerous vitamins, etc. Third, dietary supplements in the form of multivitamin tablets are inexpensive, widely-available and routinely consumed*."

COMMENT: Fortification and dietary supplements do not currently alleviate nutrient deficiencies; therefore, it is unlikely that such approaches will alleviate nutrient deficiencies in the future, particularly if they are exacerbated by lower crop nutrient density. For example, 30% of women and girls worldwide (15-49 years) currently have iron-deficiency anemia (World Health Organization, 2025), including nearly 40% of women and girls (12-21 years) in the U.S. (Weyand et al., 2023). These deficiency exist despite decades of dietary supplement availability (Macdougall, 2017).

The DOE CWG report ignores that high CO_2 concentrations affect a wide range of macro- and micronutrients beyond iron and zinc, such as lithium (for mental health), magnesium (for muscle and nerve function, blood pressure regulation, bone health, and energy production), and others (Loladze, 2014). Dietary supplements do not address these critical contributions to hidden hunger (Wallace et al., 2014). U.S. government guidelines emphasize that supplements are not an adequate substitute for a nutrient dense diet (U.S. Department of Agriculture & U.S. Department of Health and Human Services, 2010).

Further, fortification of food or use of supplements will not address the health risk posed by increased toxin concentrations in grain associated with elevated CO_2 and CO_2-induced warming.

ACTION: When discussing fortification and dietary supplements, the report authors need to add the important context described above that while these approaches have been in place for decades, they currently fail to fully alleviate nutrient deficiencies that exist within the U.S. and across the globe, and that these approaches cannot alleviate risks posed by increased toxin concentrations.

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Comment Type: Technical

DOE CWG STATEMENT: "*One concern about reliance on adaptive strategies is whether they are feasible in low-income countries. Micronutrient deficiency is already a problem in the developing world and dietary supplements have proven to be an effective low-cost response (Ebi et al. 2021)*."

COMMENT: As noted in the previous comment, micronutrient deficiency is a health problem in developing and developed countries; dietary supplements have not solved the problem. Micronutrient deficiency is expected to increase with elevated CO_2 (Beach et al., 2019; Smith & Myers, 2019; Weyant et al., 2018; Zhu et al., 2018). As supplements have been unable to fully address the current deficiencies in micronutrients, it is unlikely they will successfully ameliorate a future increased health burden caused by nutrient dilution in crops. In addition, as noted in the

previous comment, supplements will not address the health issues posed by increased toxin concentrations in grain expected with elevated CO_2 and CO_2-induced warming.

ACTION: The report authors need to remove the statement that dietary supplements have proven to be an effective low-cost response to micronutrient deficiency, or they need to find a source other than Ebi et al. (2021) to support this statement. Ebi et al. (2021) does not make this claim. Ebi et al. (2021) discuss dietary supplements but do not conclude they have “proven to be an effective low-cost response" to declining nutrient density in crops.

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Comment Type: Technical

DOE CWG STATEMENT: “*It should also be noted the IPCC emission scenarios that generate high levels of warming also involve strong income growth. The SSP scenarios 3 assume that, compared to 2005 levels, global per capita income will double by 2100 in the lowest growth case (SSP3), and in the highest emission case (SSP5) global per-capita income will grow nearly 16-fold. In that scenario even the poorest regions (Africa and the Middle East) end up with a per capita income of about US$126,000, 70 percent higher than current U.S. per capita income (about US$75,000). Consequently the same scenarios in which CO_2 levels increase the most are also those in which global poverty is largely eliminated, in which case all countries would be able to afford dietary supplements as necessary to address micronutrient deficiencies, if they arise and cannot be addressed using on-farm agricultural strategies*.”

COMMENT: The U.S. is one of the wealthier countries in the world, yet many U.S. citizens suffer from micronutrient deficiencies. Wallace et al. (2014) used data from the National Health and Nutrition Examination Survey (2007 – 2010) to determine that despite the fact that 51% of Americans consume multivitamin/mineral supplements with ≥ 9 micronutrients, many fail to meet the estimated average requirement for vitamin A (35%), vitamin C (31%), vitamin D (74%), vitamin E (67%), vitamin K (77%), calcium (39%), magnesium (46%), potassium (100%), and choline (92%). Bird et al. (2017) concluded that “Nearly one-third of the U.S. population is at risk of deficiency in at least one vitamin, or has anemia.” Growth and development of children and lifespan health depends on adequate intake of micronutrients. Decreasing the nutrient density of diets through continued emissions of CO_2 will exacerbate the situation, even with any associated income growth.

Ingestion of toxins in food is a global problem that affects the health of millions of people, including those living in wealthy countries (Consumer Reports, 2012, 2014; Gibb et al., 2019). A growth in income will not offset the health risk posed by contaminated food. It is expected that this health risk will grow as elevated CO_2 and CO_2-induced warming increase the toxin content of crops (Dhar et al., 2020; Farhat et al., 2021, 2023; Guo et al., 2011; Muehe et al., 2019; J. Wang et al., 2019; Y. Wang et al., 2023; Yuan et al., 2021).

ACTION: The report authors need to clearly state that relying on economic development in order to afford supplements will not fully address the health problems associated with declining nutrient density and increasing toxin concentrations in crops, both of which are anticipated to occur with elevated CO_2 and CO_2-induced warming. The report needs to acknowledge the

compelling body of research that documents the fact that supplements (and fortification) do not currently address nutrient deficiency or toxin exposure from food.

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Comment Type: Technical

DOE CWG STATEMENT: "*In summary, there is abundant evidence going back decades that rising CO_2 levels benefit plants, including agricultural crops, and that CO_2-induced warming will be a net benefit to U.S. agriculture.*"

COMMENT: There is abundant evidence that under ideal conditions (i.e., adequate nutrients and water and optimal temperatures), crop productivity increases with rising CO_2 levels (e.g., Ainsworth & Long, 2021). These increases diminish or disappear when conditions are non-ideal (i.e., nutrients are limited, water and temperatures are sub-optimal). As CO_2 concentrations increase, ambient temperatures will continue warming and weather patterns will shift, altering water and nutrient availability. It is not clear that boosts in productivity generated by elevated CO_2 will compensate for the damage to plant productivity caused by these other stressors. In many parts of the world, even with adaptation, crop yields are expected to decline (Hultgren et al., 2025). Further, discussed in the comments above, elevated CO_2 is associated with declining nutrient density in crops. Therefore, it is disingenuous to state that there is "abundant evidence going back decades that rising CO_2 levels benefit plants." This statement cherry-picks the productivity boost to plants associated with rising CO_2 levels while ignoring the negative human health outcomes that will co-occur.

ACTION: The authors need to remove the claim from the DOE CWG Report that "*there is abundant evidence going back decades that rising CO_2 levels benefit plants, including agricultural crops, and that CO_2-induced warming will be a net benefit to U.S. agriculture.*" The scientific consensus is that CO_2-induced climate change—including rising temperatures, shifting weather patterns, and altered precipitation—will outweigh the benefits of CO_2 fertilization, leading to net yield declines across major agricultural regions in the U.S. and globally (Auffhammer & Schlenker, 2014; Hsiao et al., 2019; Hultgren et al., 2025; Jägermeyr et al., 2021; Lobell et al., 2011; Lobell & Field, 2007; Muehe et al., 2019; Zhu et al., 2023). Further, as outlined in the comments to Section 9.4 above, CO_2-induced warming is associated with nutrient dilution and increased toxin levels in crops. It is factually inaccurate to state or imply that "CO_2-induced warming will be a net benefit to U.S. agriculture."

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Comment Type: Technical

DOE CWG STATEMENT: "*To the extent nutrient dilution occurs there are mitigating strategies available that will need to be researched and adapted to local conditions*."

COMMENT: There are strategies, such as biofortification and supplementation, that can be employed to help mitigate nutrient dilution in crops, as discussed in Ebi et al. (2020). However, as outlined in the comments above, these approaches currently do not address nutrient deficiencies and therefore it is unrealistic to expect that they will be able to address future

nutrient deficiencies exacerbated by rising CO_2 levels and crop nutrient density. Further, these strategies will not solve the problem of increased concentrations of toxins in crops associated with increased CO_2 and/or CO_2-induced warming.

ACTION: The report authors need to clarify that these mitigation strategies will only partially address the health problems associated with reduced nutrient density in crops and will not address at all the problem of increased toxin concentrations in crops.

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Public Comment on the DOE Climate Report: Managing Risks of Extreme Weather

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Comment Type: Technical, DOE report: pp. 110-111

Summary: Sections 10.1 and 10.2 of the DOE report are poorly cited (with incorrect and non-peer reviewed citations) and reach overly broad conclusions not supported by the evidence. In particular, the authors say that climate risks are primarily driven by society's ability to adapt, drawing on data on economic costs of past disasters and excluding all other economic impacts of climate change, as well as failing to examine future scenarios of risk under different warming scenarios, and how these may affect human mortality and costs of adaptation. Changes are warranted to this section to address these deficiencies.

10.1 Socioeconomic Context

STATEMENT: "The evolution of climate risk in the U.S. has been dominated by societal factors, rather than by changes to the actual weather and climate hazards." (p. 110)

RESPONSE: See the response to Section 10.2 below for a more thorough response to this section and the claims made therein as they are largely overlapping with the context of Section 10.2. While this section includes some citations of natural disaster costs and technological tools (such as weather forecasting) and other adaptation methods used to reduce or alleviate these costs, no citations are given that actually describe or discuss the trends or causes of climate risks and climate costs. As such, while this section is supposed to provide a socioeconomic context for the management of extreme weather risks, it does not. Rather, this section makes bold claims without citation while at the same time providing citations for minor, inconsequential, or tangential claims (with incorrect citations as noted below).

What this section appears to be saying is that increases in economic damages over time are a function of a wealthier society incurring more economic damage (e.g., as real estate prices rise) and that adaptation to disasters is manageable (e.g. through improved weather forecasting). Both of these claims are debatable, with many other studies (not cited by DOE) showing increasing climate risks (economic and mortality) over time with higher temperature increases (IPCC 2022), and increasing challenges and limits to adaptation (Wasley et al. 2023). For example, rising temperatures and extreme heat events reduce labor productivity and increase mortality; between 2001 and 2020, exposure to high humid heat has been estimated to have caused over 90 billion PPP$ losses per year due to decreased labor productivity in the US alone

(Parsons et al. 2022), dwarfing the estimates of some of the billion dollar disasters discussed in section 10.2. The Congressional Budget Office has also estimated that future sea level rise (of 4 feet) will cause regular flooding of residential property currently worth $930 billion (CBO 2024). The DOE report does not show how these costs will be avoided through adaptation measures, such as building codes; the Fifth US National Climate Assessment clearly indicates that while adaptation efforts have increased, there are numerous barriers that will prevent the US from adapting to many climate related changes and damages (Wasley et al. 2023).

Further, just because the US has invested in better prediction and adaptation to extreme events does not mean that we will be able to so indefinitely in the future, particularly as costs rise over time. To be most accurate, the DOE report must account for the full range of potential economic losses from climate impacts into the future, which it fails to do, including increases in mortality and morbidity, labor productivity, and declines in natural capital (Bastien-Olvera and Moore, 2020; Tol, 2023; Oda et al. 2023). For example, a (not-cited) Office of Management and Budget report has estimated that the United States gross domestic product could be reduced by 3 to 10 per cent by end of the century as a result of climate impacts, leading to annual Federal revenue loss of 7.1 per cent by the same time period, creating challenges in funding basic government functions (OMB, 2022). The Congressional Budget Office has released its own analysis, estimating a 4% reduction in GDP (CBO, 2024).

There is also an erroneous citation in this section. There is no paper by Deryugina and Hsiang (2023); a paper with the title given in the DOE report is Deryugina and Hsiang is from 2014 and is not about mortality reduction due to forecasting improvements. Deryugina and Hsiang (2014) provide economic analysis that “warmer daily temperatures will lower annual growth by 0.06-0.16 percentage points in the United States unless populations engage in new forms of adaptation”, furthering our assertion that economic costs are underestimated by the DOE report.

The DOI in the DOE bibliography for this citation (https://www.nber.org/papers/w31361) points to Shrader, Bakkensen and Lemoine (2023) which may be what the authors intended. Nonetheless, Shrader et al. (2023) state that “A large literature across economics, epidemiology, and other fields has shown that extreme temperatures are a major source of mortality”. The Shrader paper does not say that “Early warning systems, satellite monitoring, and improved weather forecasting have reduced deaths” as claimed in DOE (p. 110); Shrader et al. do say that improving forecasts (particularly for extreme heat) so that people can adapt by reducing their exposure through personal choices provides valuable economic benefits: “NWS forecasts save thousands of lives per year compared to less accurate alternatives and agents would pay billions of dollars for more accurate forecasts” (Shrader et al p. 34). Given that the current administration has cut more than 600 National Weather Service (NWS) staff positions and proposed cuts in the National Oceanic and Atmospheric Administration (NOAA) budgets, this is the opposite of what the paper suggests should be happening.

In conclusion, this section of the report must address the entirely of economic estimates of climate-related damage to the US, not limited to extreme events and disasters; discuss why the authors do not use the citations provided here; correct the incorrect citation and provide a

summary of the paper that aligns with the paper's authors own conclusions; and provide evidence and citations around the costs and limits to a range of adaptation actions, which are entirely missing.

Citations:

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Shrader, J. G., Bakkensen, L., & Lemoine, D. (2023). Fatal errors: The mortality value of accurate weather forecasts (No. w31361). National Bureau of Economic Research.

Tol, R. S. J. (2023). Social cost of carbon estimates have increased over time. Nature Climate Change, 13(6), 532–536. https://doi.org/10.1038/s41558-023-01680-x

Wasley, E. et al. 2023. Chapter 31: Adaptation. In Fifth US National Climate Assessment, US Global Change Research Program, Washington DC.

10.2 Data Challenges

STATEMENT: "[T]rends in losses from extreme weather and climate events are dominated by population increases and economic growth."

RESPONSE: The statements made in this section rely completely on one author, Roger Pielke, Jr., via one peer-reviewed paper (Pielke, Jr., 2024) and one non-peer-reviewed Substack post titled "RIP NOAA's billion dollar disasters", but this is inadequate to prove any points regarding economic damage from disasters more widely. Essentially Pielke's critique is about the way NOAA uses language regarding attribution to climate change from their 'billion dollar damages' dataset, as well as how this data was collected and adjusted (such as for inflation) in a transparent manner. What this section of the DOE report **does not show** is that disasters are decreasing or that climate change is not implicated in the rise of extreme events. Indeed, the frequency and intensity of many extreme events (including hot days and marine heat waves) are clearly increasing, with strong evidence these changes are driven by climate change (see other sections of this rebuttal report) (IPCC 2022). Indeed, in the Pielke (2024) study cited by the DOE, the author notes, "The point here is not to call into question the reality or importance of human-caused climate change – it is real, and it is important."

Further, there are numerous datasets other than NOAA's billion dollar disasters work that show the rise in climate change-related extreme events (e.g. observed trends in heat waves and extreme precipitation events, see Marvel et al. 2023). Other estimates that use attribution analysis show a clear increase in the number of extreme events attributable in part to climate change (Clarke et al. 2022) and to increased costs of damages globally (Newman and Noy 2023). Newman & Noy (2023) find that globally, climate change-attributed costs of extreme weather over 2000–2019 are estimated to be US $2.86 trillion (an average of US $143 billion per year). See additional economic damages in table 19.1 of the Fifth US National Climate Assessment (Hsiang et al 2023). Another recent paper estimated that mortality from tropical cyclone events in the US is widely underestimated, and that each event likely is responsible for indirect excess mortality in the range of 7,000-11,000 deaths per event (Young and Hsiang 2024).

Finally, it is notable that the NOAA "Billion Dollar Disaster" product is not discussed in detail at all, nor is there any representation of the scientific and technical debate or discussion. A primary citation for this database (Smith and Katz, 2013) is completely omitted from this section. Further,

the billion dollar dataset is likely an underestimation of climate damages, as it does not include environmental and ecosystem damages outside of agriculture and forestry, while the Fifth National Climate Assessment chapter 8 notes widespread risks to species and ecosystems which high economic and cultural values (McElwee et al. 2023). The DOE report statement that adaptation measures have “have lowered economic losses relative to GDP” (p. 111) is also unsubstantiated (no citation is provided), and is disputed by analysis by OMB and CBO cited above that looks are a wider range of economic losses than just those from extreme events and projects increasing costs and damages into the future (which the DOE report does not do).

Taken as a whole, this section is highly under-cited, and instead relies almost exclusively upon a single author, providing only a ‘cherry picked’ view. The representation of state-of-the-science attribution discussion and debate is completely omitted. The authors of this section must address deficiencies in citations, provide additional evidence for any claims that they make regarding the NOAA dataset and lowered economic losses relative to GDP attributable to adaptation measures, and address the existence of numerous other empirical datasets that show increased extreme events attributable to climate change (particularly heat days, marine heat waves, and extreme precipitation),

Citations:

Clarke, B et al. 2022 Extreme weather impacts of climate change: an attribution perspective *Environ. Res.: Climate* 1 012001

Hsiang et al. (2023) Chapter 19: Economics. In Fifth National Climate Assessment, US Global Change Research Program, Washington DC.

IPCC, 2022: Summary for Policymakers [H.-O. Pörtner, D.C. Roberts, E.S. Poloczanska, K. Mintenbeck, M. Tignor, A. Alegría, M. Craig, S. Langsdorf, S. Löschke, V. Möller, A. Okem (eds.)]. In: Climate Change 2022: Impacts, Adaptation and Vulnerability. Contribution of Working Group II to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [H.-O. Pörtner, D.C. Roberts, M. Tignor, E.S. Poloczanska, K. Mintenbeck, A. Alegría, M. Craig, S. Langsdorf, S. Löschke, V. Möller, A. Okem, B. Rama (eds.)]. Cambridge University Press, Cambridge, UK and New York, NY, USA, pp. 3–33, doi:10.1017/9781009325844.001.

Marvel, K. et al. 2023. Chapter 2: Climate Trends: In Fifth US National Climate Assessment, US Global Change Research Program, Washington DC.

McElwee, P et al. 2023. Chapter 8: Ecosystems, biodiversity and ecosystem services. In Fifth US National Climate Assessment, US Global Change Research Program, Washington DC.

Newman, R., & Noy, I. (2023). The global costs of extreme weather that are attributable to climate change. *Nature Communications*, *14*(1), 6103. https://doi.org/10.1038/s41467-023-41888-1

Pielke Jr., R. (2024). Scientific integrity and U.S. 'billion dollar disasters'. npj Natural Hazards, 1, Article 12. https://www.nature.com/articles/s44304-024-00011-0

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Young, R., Hsiang, S. (2024) Mortality caused by tropical cyclones in the United States. *Nature* 635, 121–128. https://doi.org/10.1038/s41586-024-07945-5

Comments below about text summary and data graphic presented in section 10.2 of the DOE report.

Edited by A.E. Dessler, Texas A&M University

Page number: p. 111

Comments focus on Figure 10.1: “Losses per disaster as a % of Gross Domestic Product in NOAA’s billion-dollar disaster dataset (the version downloaded in July 2023), 1980 to 2022.”

Comment type: technical

(1) **introduction:** Section 10.2, Figure 10.1 comments directly on the U.S. Billion-dollar disaster research conducted at NOAA.

The reality of U.S. disaster losses is much more complex and nuanced than Figure 10.1 indicates. The frequency and cost of weather and climate disasters are increasing in the United States due to a combination of increased physical exposure, vulnerability, and hazards, the latter of which have been impacted by climate change via increasing the frequency and intensity of some types of extremes that lead to billion-dollar disasters:

> "The intensification of these and other more extreme weather-climate events are intersecting with areas experiencing high levels of health disparities, social vulnerabilities, and increased exposure due to population growth in hazard-prone areas." (National Academies of Sciences, Engineering, and Medicine2024)

The following sections highlight counterpoints on why the section 10.2, Figure 10.1 chart is presenting a misinformed view of U.S. disaster impacts.

(2) **background section:** The reality of U.S. disaster losses is much more complex and nuanced than the misleading Figure 10.1 shown in section 10.2. The following are examples that more clearly highlight the reality of U.S. disaster impacts.

As a first point, the figure 10.1 excludes U.S. disaster event data for 2023 and 2024, which have the highest and second highest count of billion-dollar disasters (inflation-adjusted) on record, 28 and 27, respectively (Smith, 2025). The average length of time between disasters fell from 82 days during the 1980s to 19 days during the last 10 years (2015-2024). Shorter time intervals between disasters can mean less time and resources available to respond, recover, and prepare for future events. This

increased frequency of events produces cascading impacts that are particularly challenging to vulnerable socioeconomic populations.

(3) **analysis:** Regional, state, or county-level discussions of the changing nature of risks and the numerous drivers are important for on-the-ground decision making to improve resilience. As Figure 10.1 in section 10.2 is at a national level, it is not particularly useful or illuminating for understanding the changing nature of weather and climate risk.

Secondly, Figure 10.1 shows disaster costs have a slightly negative growth rate over time when compared to the U.S. GDP. Given the more localized scale of extreme events, it is more informative to examine state GDP vs. state cost from billion-dollar disaster events. In any given year there are numerous states with billion-dollar disaster loss costs as a percentage of state GDP in the 2%-20% range (NOAA, 2025). In contrast, the inflation-adjusted cost per capita has increased from approximately $90 per capita in the 1980s to more than $400 per capita in the late 2010s and has remained at a high level in recent years (Smith, 2025).

There are several explanations for these trends, including increases in exposure (i.e., more assets at risk); increases in vulnerability (i.e., the amount of damage a hazard of given intensity, such as high winds, can cause at a location); and increases in the frequency and intensity of some types of extreme events due to human-caused climate change (USGCRP 2023). Regarding increased exposure, much of this growth and property development has continued in high-risk areas like coasts, the fire-prone wildland-urban interface in the West, and river floodplains. Areas where building codes are insufficient for reducing damage from extreme events are especially vulnerable to more extreme weather.

But we also know from extreme event attribution research that human-caused climate change is increasing the frequency and intensity of certain types of extreme weather that lead to billion-dollar disasters—most notably the rise in drought intensity in the western states, lengthening wildfire seasons in the western states, the potential for extremely heavy rainfall becoming more common in the eastern states and sea level rise, which is worsening hurricane storm surge flooding (please see responses to corresponding sections on drought, fire, extreme precipitation, sea level rise, and hurricanes and tropical cyclones).

As a result, over the last ten years (2015-2024), the U.S. has been impacted by 190 separate billion-dollar disasters that have killed more than 6,300 people (direct and indirect fatalities) and cost ~$1.4 trillion in damage (Smith, 2025).

The U.S. has also been impacted by landfalling category 4 or 5 hurricanes in six of the last eight years (Harvey, Irma, Maria, Michael, Laura, Ida, Ian, Helene), which is among the highest frequencies of major hurricane landfall on record (NOAA/AOML, 2025). In 2024, the U.S. impacts from Hurricanes Helene and Milton alone were particularly destructive, causing more than $100 billion in combined damage across Florida, Georgia, South Carolina, North Carolina, Tennessee, and Virginia over a two-week period. A number of these hurricanes are rapidly intensifying with record warm sea surface temperatures and causing flooding and wind damage many hundreds of miles inland - Ida in 2021 and Helene in 2024 (please see response to Section 6.2 regarding hurricanes).

Also, there were more billion-dollar extreme rainfall-induced flood events from 2010-2024 (26) than in the 1980s, 1990s and 2000s combined (19) - all flood events CPI-adjusted to 2024 dollars (NOAA, 2024). While increased population in or near floodplains has contributed to rising flood-related damages, a warming atmosphere - driven by climate change - is also a key factor. Warmer air holds more moisture, which raises the likelihood of extreme precipitation events and severe flooding (please see response to Section 6.4 regarding extreme precipitation).

To capture these growing trends, the NOAA billion-dollar disaster dataset represents the nation’s authoritative database on major disaster costs incorporating the most robust public and private sector data sources and analysis. In performing these disaster cost assessments these statistics were developed using the most comprehensive public and private sector sources and represent the estimated total costs of these events -- that is, the costs in terms of dollars that would not have been incurred had the event not taken place. More than one dozen public and private sector data sources help capture the total, direct costs (both insured and uninsured) of the weather and climate events. These costs include: physical damage to residential, commercial, and municipal buildings; material assets (content) within buildings; time element losses such as business interruption or loss of living quarters; damage to vehicles and boats; public assets including roads, bridges, levees; electrical infrastructure and offshore energy platforms; agricultural assets including crops, livestock, and commercial timber; and wildfire suppression costs, among others.

(4) **action**: The DOE CWG authors should approach section 10.2 and Figure 10.1 using a more regional or state-based approach, and should examinethe losses by inflation-adjusted, per-capita costs, which are increasing nationally, but will show variance state-by-state over time.

(5) **conclusion:** Even the most complete estimates should be considered economically conservative with respect to what is truly lost but cannot be fully measured due to a lack

of consistently available data. For example, no prominent national or global hazard databases consider losses to: natural capital or environmental degradation; mental or physical healthcare related costs; include the value of a statistical life (VSL); or supply chain, contingent business interruption costs.

References:

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National Academies of Sciences, Engineering, and Medicine. 2024. Compounding Disasters in Gulf Coast Communities 2020-2021: Impacts, Findings, and Lessons Learned. Washington, DC: The National Academies Press. https://doi.org/10.17226/27170

NOAA National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disasters (2025). https://www.ncei.noaa.gov/access/billions/, DOI: 10.25921/stkw-7w73

Smith, A., 2025: “2024: An active year of U.S. billion-dollar weather and climate disasters,” Annual Report, (2024: An active year of U.S. billion-dollar weather and climate disasters | NOAA Climate.gov)

U.S. Global Change Research Program. (2023). Fifth National Climate Assessment (A. R. Crimmins, C. W. Avery, D. R. Easterling, K. E. Kunkel, B. C. Stewart, & T. K. Maycock, Eds.). U.S. Global Change Research Program. https://doi.org/10.7930/NCA5.2023

Pielke Jr., R. (2024). Scientific integrity and U.S. 'billion dollar disasters'. npj Natural Hazards, 1, Article 12. https://www.nature.com/articles/s44304-024-00011-0

Response to Section 10.3

Lyssa Freese, Ph.D., Michael Allen, Ph.D., Kristie L. Ebi, Ph.D., MPH, Mary H. Hayden, Ph.D., J. Jason West, Ph.D.

Summary

Below are technical comments to statements made within Chapter 10 of the DOE CWG Report. The page numbers associated with each statement/comment grouping indicates the page on which the statement starts.

Here we respond to the section “10.3 Mortality from temperature extremes”, providing a critical review of the statements and literature therein. This refers to the section found on pages 111-114 of the DOE CWG report.

This section makes the case that mortalities due to a changing climate are impacted by both changes in heat and cold extremes, and that adaptation needs to be considered in estimations of the mortality impacts. However, it relies upon a number of mis-representations of the current scientific understanding of human impacts of heat and cold. It also contradicts itself in the capacity of people to adapt. Lastly, this section focuses only on mortality impacts of heat and cold, neglecting the many other pathways by which climate change can affect both mortality and morbidity, for which there is an abundance of evidence.

10.3.1 Heat and cold risks

Comment 1

Original text:
Page 111 states ”It stands to reason that extreme heat events would likely become more frequent while extreme cold events would become less frequent.”

Comment type: Technical

There is no scientific support provided for this statement.

Action:

Provide the scientific support for this statement.

Comment 2

Original text:
Page 112 states “Mortality during heat extremes is typically caused by heat stroke and heat exhaustion, while mortality during cold extremes typically stems from hypothermia and heart strain. Global mortality is substantially greater for cold conditions than for hot conditions (Zhao et al 2021, Ritchie 2024).”

Comment type: Technical
The second sentence, “Global mortality is substantially greater for cold conditions than for hot conditions” could be read as being about extremes, which could mislead readers.

The second sentence also does not convey the reason for this pattern, as explained in Alahmed et al. 2025. This also applies to other statements on page 112. As stated in Alahmed et al.:

> *“*The pattern of higher cold-related deaths is primarily due to the specific form of the exposure–response relationship. For temperature and mortality, this relationship is non-linear, often described as U-shaped or J-shaped, indicating that both temperatures that are too cold and too hot increase mortality risk, separated by a comfortable or optimal temperature. In epidemiological studies, this optimal temperature is referred to as the minimum mortality temperature—ie, the point where deaths are lowest. From an epidemiological point of view, anything above this optimal temperature can be considered hot for a given location, whereas those lower are considered cold, which differs from people’s everyday sense of what constitutes a hot or cold day. For example, the optimal temperature often sits quite high within a location’s annual temperature range, meaning that days classified as cold in epidemiological terms could feel mild or even warm to some people.”

Using analyses from Barcelona as an example, Alahmed et al. continue:

> “One explanation for why cold deaths were higher than heat deaths is mathematical: the area under the curve for heat is only a third of the total area ... Using numerical integration, the area under the curve was 2·745 for temperatures higher than 23·6°C and

5·287 for temperatures lower than 23·6°C. This finding partly explains why cold-related deaths are higher than hot-related deaths; however, it does not give the whole picture.

Looking at each day from 2000 to 2018 (6940 days) in Barcelona, most days had temperatures lower than the optimal temperature. When rounding temperatures to full digits, we see that the frequency of cold days (<23·6°C) far exceeds hot ones (>23·6°C; appendix p 1). Put another way, the figure … shows that the contribution of cold days on attributable mortality is much larger than the contribution of a much more modest count of hot days.

However, what about the influence of temperature on the actual risk of death on a given day irrespective of how often it happens? We can assess the relative risk compared with the optimal temperature. During the study period in Barcelona, the coldest day had an average temperature of 1·4°C. On this day, the risk of cardiovascular death increased by 50% (relative risk 1·5) compared with the optimal temperature. Conversely, the hottest day had an average temperature of 32·1°C, which was associated with a 240% increase in cardiovascular death risk (relative risk 2·4). Such a day, however, was very rare, occurring only once in 19 years in Barcelona. Although the relative risk of death was higher on the hottest day, such extreme heat days were so infrequent that the attributable or excess deaths from heat remained much lower than those from more frequent cold. If very hot days were to occur more frequently, the excess deaths related to heat would be considerably higher."

In short, the finding of greater cold-related mortality can be an outcome of the mathematical approach used, and results in part because of the greater frequency of cold conditions. In fact, rates of mortality are greater on hot days. The implication of the DOE CWG that climate change will not detrimentally affect mortality is not supported in recent science.

Action:
In order to address this issue, the DOE CWG needs to accurately include in the text the reasons for more cold-related mortality (e.g., a consequence of the mathematical approach used) and that mortality rates on hot days are higher. They also need to adjust the subsequent arguments made in this section to reflect this outcome.

Comment 3

Original text:

Page 112 of the DOE CWG states: “The U.S. EPA (citing data from the Centers for Disease Control) reports that on average over 1999 to 2015 there were 2.2 deaths per million Americans for which cold was listed as the main underlying cause and an additional 2.4 deaths per million for which cold was listed as a contributing factor (EPA 2025).”

Comment type: Editorial, technical

The DOE CWG highlights the greater burden of cold-related than heat-related mortality in the United States. The DOE CWG argues that climate change will positively impact overall mortality by reducing cold-related mortality even as heat-related mortality increases with climate change.

A more relevant text on cold-related mortality is the Liu et al. 2025 study that used CDC data to show that annual cold-related age-adjusted mortality rates in the US increased 3.4% (95% CI 2.4% to 4.3%) over the period 1999 to 2022, with a nonsignificant increase from 1999 to 2017, followed by a significant annual increase from 2017 to 2022 of 12.1% (95% CI, 6.3% to 27.1%). In other words, the DOE CWG report selected a period to report during which there was no significant trend in cold-related mortality. See figure below:

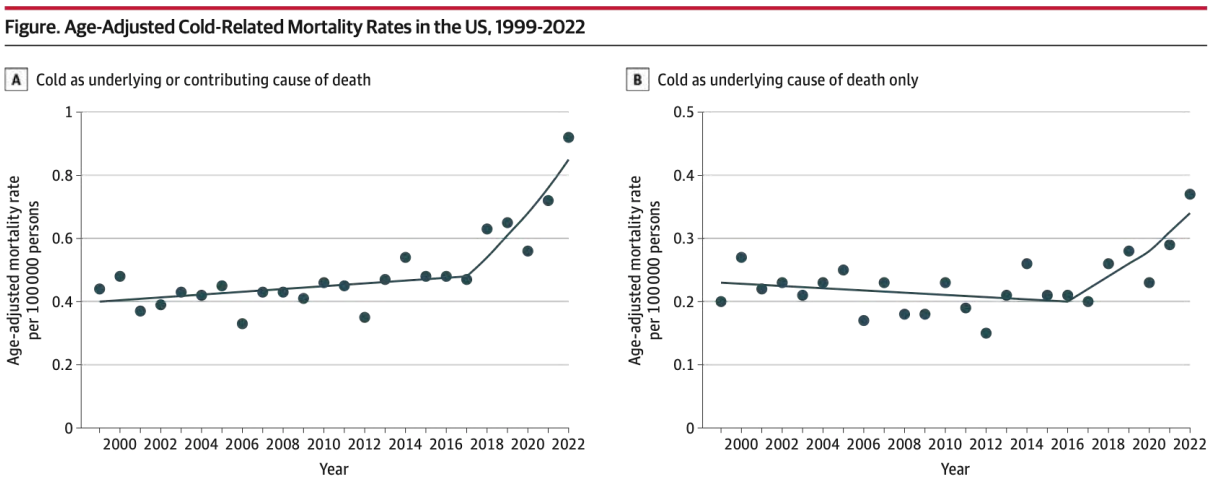
Figure. Age-Adjusted Cold-Related Mortality Rates in the US, 1999-2022
A Cold as underlying or contributing cause of death
B Cold as underlying cause of death only
Age-adjusted mortality rate per 100 000 persons
Year
Mortality rates were age-adjusted to the US population in 2000. Cold-related mortality rates are plotted as dots, with a Joinpoint model plotted as a line and a single Joinpoint in either 2017 (panel A) or 2016 (panel B).

Image from Liu, M., Patel, V. R., & Wadhera, R. K. (2025). Cold-Related Deaths in the US. *JAMA*, *333*(5), 427-429.

Also, this short time series does not support the DOE CWG contention that cold-related mortality is declining with climate change (Comment 1).

Action:

Correct the DOE CWG text to include information on the time series from 1999 to 2022, including that there was a significant 12.1% increase in cold-related mortality over the period 2017 to 2022, which is inconsistent with the argument that cold-related mortality is declining with climate change.

Comment 4

Page 112 states: “Figure 10.2 shows the distribution of results from Gasparini et al. (2015) by country. For the United States the fraction of deaths attributable to temperature was 5.9 percent, of which 5.5 percent was due to cold, thus cold weather killed 14 times as many people as hot weather.”

Comment type: Technical

Figure 10.2 does not consider climate change; these are historic observations over about twenty years, ending about in 2020, depending on the country.

Action:
Include in the DOE CWG report that the figure provides historic context only and does not provide any information or insights into current and future mortality during cold and hot periods in the United States.

Comment 5

Original content:

On pages 111-114, the DOE CWG focused *only* on mortality risk due to temperature extremes, a small component of the health risks due to climate change.

Comment type: Editorial

Heatwaves are associated with stark elevations in not only mortality but also morbidity. This report is missing health and well-being impacts such as the detrimental impacts on older adults, maternal health, changes in food security and malnutrition, changes in the geographic range and seasonality of climate-sensitive infectious diseases, detrimental impacts on mental health

and well-being, increased exposure to air pollution, increased exposure to wildfires and related pollution, and mixed impacts on communicable and non-communicable disease. Other references provide abundant evidence that climate change is worsening health risks by these many pathways globally and in the US (IPCC 2022, Baharav et al. 2023, Jiao et al. 2023, Kuehn et al. 2017, Zhang et al. 2023, Solomon and LaRocque 2019, Boscarino et al. 2014, Obradovich et al. 2018, Zhong et al. 2018, Orru et al. 2017, Hayden et al. 2023, Law et al. 2025). Lacking discussion of these many other health impacts gives the impression that broader conclusions about climate impacts on health are brought into question by the DOE CWG concerns over temperature-related mortality, when in fact the report says nothing about other climate-health pathways.

Action:

To address this concern, the DOE CWG needs to broaden this discussion to include the literature on health impacts of climate change in more ways than heat and cold extremes and their impact on mortality, and to put into context that their criticisms of temperature-related mortality estimates address a small component of the overall health effects of climate change. This at least includes the impacts on older adults, vector borne diseases, maternal health, mental health, well-being, food security and malnutrition, pollution and health, and exposure to wildfires and health.

Comment 6

Original text:

Statement 1, Page 113: “In the context of large declines in heat-related mortality, rising temperatures are associated with a net saving of lives since they reduce mortality from cold events. AR6 Working Group 2 Chapter 16.2.3.5 (O’Neill et al. 2022) acknowledges that heat-related mortality risk is declining over time:

> Heat-attributable mortality fractions have declined over time in most countries owing to general improvements in health care systems, increasing prevalence of residential air conditioning, and behavioral changes. These factors, which determine the susceptibility of the population to heat, have predominated over the influence of temperature change.

Yet the IPCC misrepresents the overall situation in its AR6 Synthesis report. Section A.2.5 of that document states: “In all regions increases in extreme heat events have resulted in human mortality and morbidity (very high confidence).” But it is silent on the larger decline of deaths during extreme cold events.”

Comment type: Technical, Reference

While declines in heat-related mortality have been observed in some regions, studies indicate this reduction has not been geographically homogeneous, and in some instances slowed or even reversed (Sheridan et al. 2021). And while population susceptibility to heat has changed, the extent to which and sustainability of individual adaptive capacity remains uncertain in the context of a warming world (Arbuthnott et al. 2016).

The DOE CWG report needs to consider more than the Synthesis report of the IPCC AR6; WGII Chapter 7 has more information on the relative impacts of heat and cold, including:

> "Wide ranging knowledge regarding the specific detection of heat- and cold-related mortality/morbidity and its attribution to observed climate change is lacking. Although there has been an observed increase in winter-season temperatures for a number of regions, to date there is variable evidence for a consequential reduction in winter mortality and susceptibility to cold over time due to milder winters; some countries demonstrate decreasing trends, while other countries show stable or even increasing trends in cold-attributable mortality fractions over time (e.g., Arbuthnott et al. (2020); Åström et al. (2013); Diaz et al. (2019); Hajat (2017); Hanigan et al. (2021); Lee et al. (2018b))."

And

> "Some research has found a negative effect of extreme cold on well-being (Yoo et al., 2021); increasing winter temperatures associated with climate change could serve to compensate for the impact of increased summer temperatures. However, the effect of high temperatures is typically found to be stronger than the effect of low temperatures, and in some cases no detrimental impacts of cold weather are found (Almendra et al., 2019; Mullins and White, 2019)."

Furthermore, the DOE CWG states that there is a "larger decline of deaths during extreme cold events", but this is not backed by any scientific citations. In fact, "a recent health impact projection study in more than 800 cities in Europe showed that a future reduction of cold deaths is unlikely to offset the projected increase in heat deaths." (Alahmed et al., 2025).

Action:
To address the concerns above, the DOE CWG needs to correct its selective quote of the IPCC, and expand and detail a more holistic impact of heat and cold on human health. The CWG needs to incorporate the results of the research cited here, including work that shows reduction of cold deaths is unlikely to offset projected increase in heat deaths.

Comment 7

Original text:

Statement 2, page 113: "The observed decline in U.S. heat-related mortality has been specifically attributed to adaptation. Wang et al. (2018) exploited the spatial variability of heatwave-related mortality across 209 U.S. cities from 1962 to 2006. While simple correlation appeared to imply an increase in mortality risk during heatwaves, accounting for adaptation to heatwave intensity caused the effect to fall to near zero and become statistically insignificant. They used the results of their epidemiological model to project heat-related mortality out to 2050 under four RCP warming scenarios (including RCP8.5) with and without adaptive behavior. Assuming people continue to adapt to the heatwave risks in the regions in which they live, Wang et al. (2018) project not only no increase in heat-related mortality, but an overall mortality decrease for the U.S. They conclude that ignoring adaptation would result in a substantial overestimate of future mortality related to heat waves… Accounting for adaptation, the overall heat-related mortality by 2050 would not change substantially over time compared to 2006."

Comment type: Technical, Reference

This is a selective decision to not include the followup sentence on this statement that says:

> "Ignoring adaptation in projections would result in a substantial overestimate of the projected heat wave-related mortality (by 277–747% in 2050). Incorporating the empirically estimated adaptation into projections would result in little change in the projected heat wave-related mortality between 2006 and 2050. **This differs regionally, however, with increasing mortality over time for cities in the southern and western U.S. but decreasing mortality over time for the north**."

With the part that was left out of the text in bold. This work does not show a uniform decrease in mortality, but rather that it is location dependent, due to, in part, the saturation and use of air conditioning. This work shows that there is a saturation level for adaptation– places already saturated with air conditioning see increases in mortality because they cannot adapt out of the impacts from heat, providing a limit to adaptation. Furthermore, this work only uses temperature related heat, while the most up to date literature emphasizes the inclusion of humidity (Baldwin et al. 2023; Vecellio et al. 2023; Buzan and Huber 2020). As the atmosphere warms, evaporation of water increases, leading to higher humidity across much of the globe,

modifying the impact of humidity on the heat-health relationship. See Figure 3, below, from their work that clearly shows increases in mortality throughout multiple regions in the United States, particularly in regions such as the Northwest, Southwest, West, where population growth is expected in the future (Cooper Center). With increases in the frequency and intensity of heat, mortality can still be identified in hot periods, irrespective of season or geography.

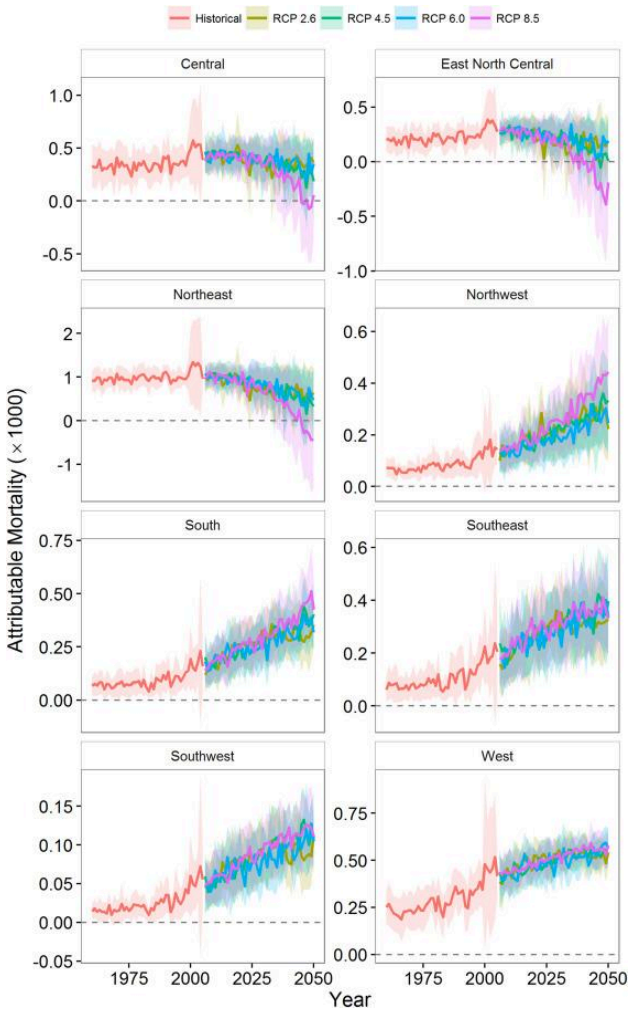

Figure from Wang, Y., Nordio, F., Nairn, J., Zanobetti, A., & Schwartz, J. D. (2018). Accounting for adaptation and intensity in projecting heat wave-related mortality. Environmental research, 161, 464-471. "Fig. 3. Projected mortality attributable to heat waves in 1960–2050 under historical, RCP 2.6, RCP 4.5, RCP 6.0, and RCP 8.5 scenarios in each of the eight climate regions: a. Northeast, b. Central, c. East North Central, d. Northwest, e. South, f. Southeast, g. Southwest, and h. West. The models considered adaptation but ignored intensity (Model 3). The solid lines and the shaded areas represent mean ± standard deviation across the predicted values using all climate models."

Action:

To address this issue, the DOE CWG needs to correct its selective quote of the article, and address the spatial heterogeneity in the mortality outcomes from the work cited, as is shown in the figures attached.

Comment 8

Original text:

Statement 3, page 112: “There is strong evidence that people adapt to weather risks. Lee and Dessler (2023) reported that 86 percent of temperature-related deaths across 40 cities in the U.S. were due to cold-related mortality, and that due to adaptation the relative risk of death declined in hot and cold cities alike as seasonal temperatures increased. Allen and Sheridan (2018) found that short, early-season cold events were 2 to 5 times deadlier than hot events, but the mortality risk of both cold and hot extremes drops to nearly zero if the events occur late in the season.”

Comment type: Technical, References

While people may adapt to weather risks, questions remain as to the capacity to adapt. Populations without access to air conditioning or individuals with chronic illness differ in personal adaptive capacity. Population health is not the same as individual health. The above reference misrepresents the methodological approaches and conclusions of Allen and Sheridan (2018) as no direct comparison between heat and cold events was conducted. There is some evidence that extreme temperature events in the early season have a disproportionately large human impact compared to those later in the season (Anderson and Bell 2011; Ng et al. 2014), with one suggested reason being a lack of acclimatization. However, to claim thermal risk becomes near zero later in the season — as heatwaves become longer-lasting — is inaccurate.

Action:

To address these concerns, the DOE CWG needs to correct the misrepresentation of the Allen and Sheriden (2018) work. It also needs to include information about the capacity and limitations to both population- and individual-scale adaptation

10.3.2 Mortality risks and energy costs

Comment 1

Original text:

Statement 1, page 114 “On very hot days (>30°C) electricity spending in high-income households rises by 0.5 percent but does not change at all in low-income households. The latter result is observed even in subsamples where all households have AC. Cong et al. (2022) report similar findings for a sample of households in Arizona. The implication is that even with widespread adoption of home heating and cooling systems, inability to afford energy leaves low-income households exposed to weather extremes.”

Comment Type: Technical, Editorial

The statements made here, which are that “an inability to afford energy leaves low-income households exposed to weather extremes” should clearly be used in reference back to the former section (10.3.1), specifically the citation of Wang et al. (2018), which assesses the impact of adaptation on heat and cold related mortalities. As the authors suggest here, if adaptation is unaffordable due to high energy costs, then we can look at the outcome from Wang’s work for the impact of heat on mortalities without adaptation (see Figure 2a below), which shows a U.S. mean increase in attributable mortalities. Wang et al. (2018) do not provide a regional breakdown (otherwise we would show that here).

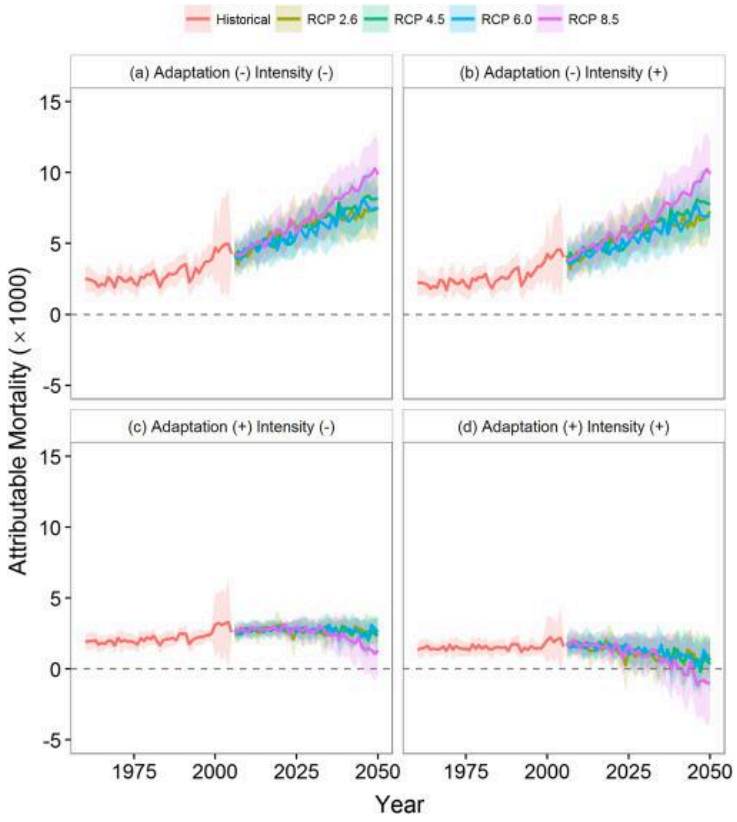

Figure from Wang, Y., Nordio, F., Nairn, J., Zanobetti, A., & Schwartz, J. D. (2018). Accounting for adaptation and intensity in projecting heat wave-related mortality. Environmental research, 161, 464-471. "Fig. 2. Projected mortality attributable to heat waves in 1960–2050 under historical, RCP 2.6, RCP 4.5, RCP 6.0, and RCP 8.5 scenario. a. Model 1: neither adaptation nor intensity was considered; b. Model 2: adaptation was considered but intensity was ignored; c. Model 3: intensity was considered but adaptation was ignored; d. Model 4: both adaptation and intensity were considered. The solid lines and the shaded areas represent mean ± standard deviation across the predicted values using all climate models."

If the authors are going to cite the inability for populations to adapt due to the cost of adaptation, they need to take that into consideration when claiming that populations will adapt to a changing climate. This section is a contradiction to the arguments these authors made in 10.3.1, and to fix this the authors need to incorporate the potential inability to adapt due to financial circumstances into their assessment in 10.3.1.

Action:

In order to address our concerns here, the DOE CWG needs to include these conclusions that adaptation may be cost-prohibitive into their logic and argument in section 10.3.1 that people

can completely adapt their way out of a changing climate. Additionally, including evidence of the limitations on capacity to adapt is needed.

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Section 10.3: Mortality from Extreme Temperatures

Andrew E. Dessler, Texas A&M University

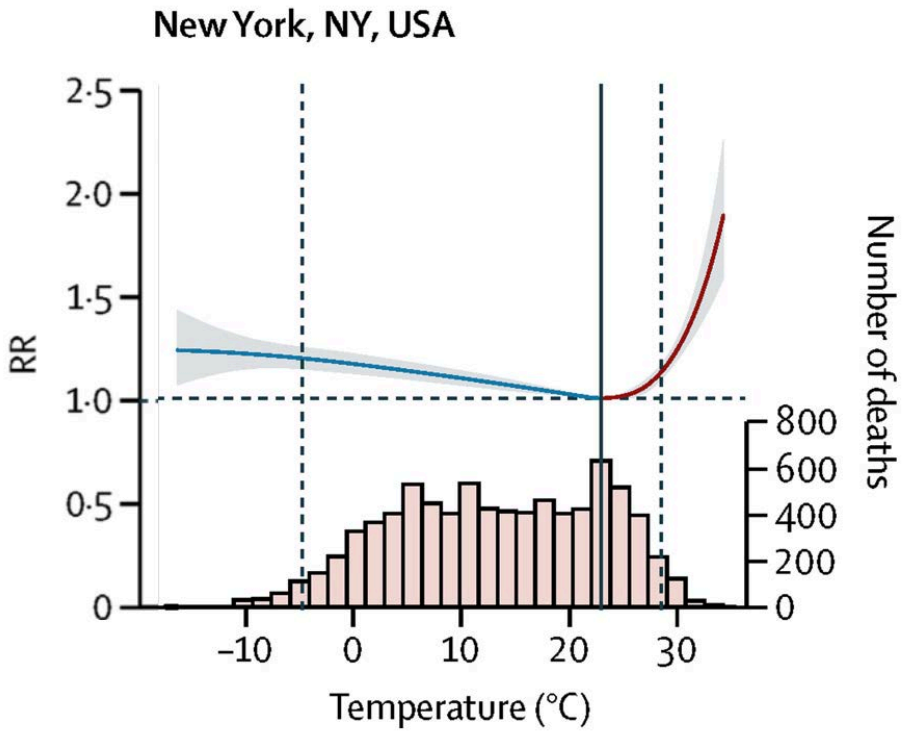
Technical Comment on Section 10.3.1, Heat and Cold Risks (pp. 111-113)

Error: The document's focus on mortality from extreme heat neglects the substantial burden of nonfatal health impacts, such as heat exhaustion, kidney disease, and cardiovascular strain. As Flouris et al. (2018) note, "Occupational heat strain (i.e., the effect of environmental heat stress on the body) directly threatens workers' ability to live healthy and productive lives." The literature connects heat exposure to increased risks of kidney disease (Flouris et al., 2018; Johnson et al., 2019), cardiovascular disease (Liu et al., 2022), as well as an expanding range of vector-borne diseases (Rocklöv & Dubrow, 2020).

Recommended Fix: Expand the assessment of health impacts to include morbidity and nonfatal outcomes, referencing comprehensive sources such as the Lancet Countdown (Romanello et al., 2024), to present a fuller picture of climate-related health risks.

Geographic Disparities in Temperature-Mortality Relationships

Error: While the document recognizes the U-shaped relationship between temperature and mortality, it under-represents both the asymmetry of this curve and the geographic differences. The increase in mortality associated with increasing temperatures is generally more pronounced than that associated with cold:



Adapted from Fig. 1 of Gasparrini et al. (2015).

In this example, slight warming in New York may initially reduce cold-related deaths, but with enough warming, the rise in heat-related deaths overtakes these gains, resulting in net increases in mortality. For the U.S. overall, warming up to ~3°C roughly balances heat- and cold-related deaths, yet beyond that threshold, heat mortality rises sharply (Lee & Dessler, 2023) unless there is significant adaptation, which is uncertain (see comment below).

Recommended Fix: Provide geographically detailed temperature-mortality analyses, clarifying how net mortality shifts with warming in different regions. Indicate thresholds (e.g., at ~3°C above preindustrial levels) at which heat-related deaths begin to outweigh reductions in cold-related mortality, and explicitly note that less-adapted populations face fewer potential benefits and greater risks.

Technical Comment on Section 10.3.1, Mortality Risks and Energy Costs (pp. 113-114)

Error: Emphasizing air conditioning (AC) as the primary adaptation to heat overlooks important practical limitations. Millions of outdoor workers (e.g., in agriculture, construction, and delivery services) cannot rely on AC for protection. Additionally,

installing and operating AC systems in places where it was previously unnecessary involves significant capital and electricity costs, which are especially burdensome for economically vulnerable populations. Dessler (2025) estimated that climate change increased annual electricity costs by $80 per person in Texas in 2023 (compared to a mid-20th century climate), reflecting the need for increased air conditioning (Dessler, 2025).

Given the track record of our society, adaptation seems like a bad bet. For example, a bill to air condition prisons in Texas failed in the Texas Legislature this year, continuing a 20-year string of failure. There is no clearer place where adaptation needs to occur, yet the failure year after year suggests that those with money just don't want to spend it helping the poor adapt.

Another example is the Low Income Home Energy Assistance Program (LIHEAP), which has historically helped vulnerable households maintain safe indoor temperatures. However, the current administration has moved to eliminate LIHEAP, firing program staff in April 2025 and proposing zero funding for 2026. Such policies directly undermine the DOE document's core assumption that continued adaptation is a certainty that will prevent mortality increases.

Recommended Fix: More realistically assess adaptation strategies by discussing the economic and institutional barriers to widespread AC deployment, and acknowledge populations (e.g., outdoor workers) for whom this strategy is not feasible.

Technical Comment on Section 10.3.2, Energy Source Costs

Error: The unstated assumption that fossil fuels must be relied upon to provide cheap electricity for AC is outdated. Renewables, such as wind and solar, now often provide the lowest-cost electricity. Focusing exclusively on "net zero" scenarios misses the opportunity to explore a range of economically and technically viable pathways to decarbonization.

Recommended Fix: Review and present up-to-date research on the costs of decarbonizing the energy sector, including multiple decarbonization levels (e.g., 60%, 75%, 90%, and 100%) rather than solely net-zero scenarios. Clarify that maximizing affordable, climate-safe renewable energy supports both adaptation and mitigation goals.

Conclusion

This section, which argues that extreme heat is not a significant health concern, rests on several important errors:

- It ignores health impacts other than death from extreme temperatures. An enormous amount of research has connected climate change to human health impacts and this needs to be reviewed.
- Their analysis of mortality glosses over the fact that the balance between lives saved and lives lost depends on how much warming occurs and what region you are testing. With small amounts of warming, some regions will win and others will lose. As warming increases, fewer places win and more lose until, with enough warming, everyone is a loser. The revised document needs to flesh out these details.
- The reliance on adaptation is a dangerous delusion. It conveniently ignores millions of outdoor workers and the prohibitive capital and energy costs for the economically vulnerable. Even in the U.S., the current administration is cutting funding that could be used to help people adapt.
- Regardless of anything else, we do need to enhance access to air conditioning. This requires producing electricity at the lowest cost, which means we should be building as much wind and solar as possible.

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A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate: A Response to Chapter 11

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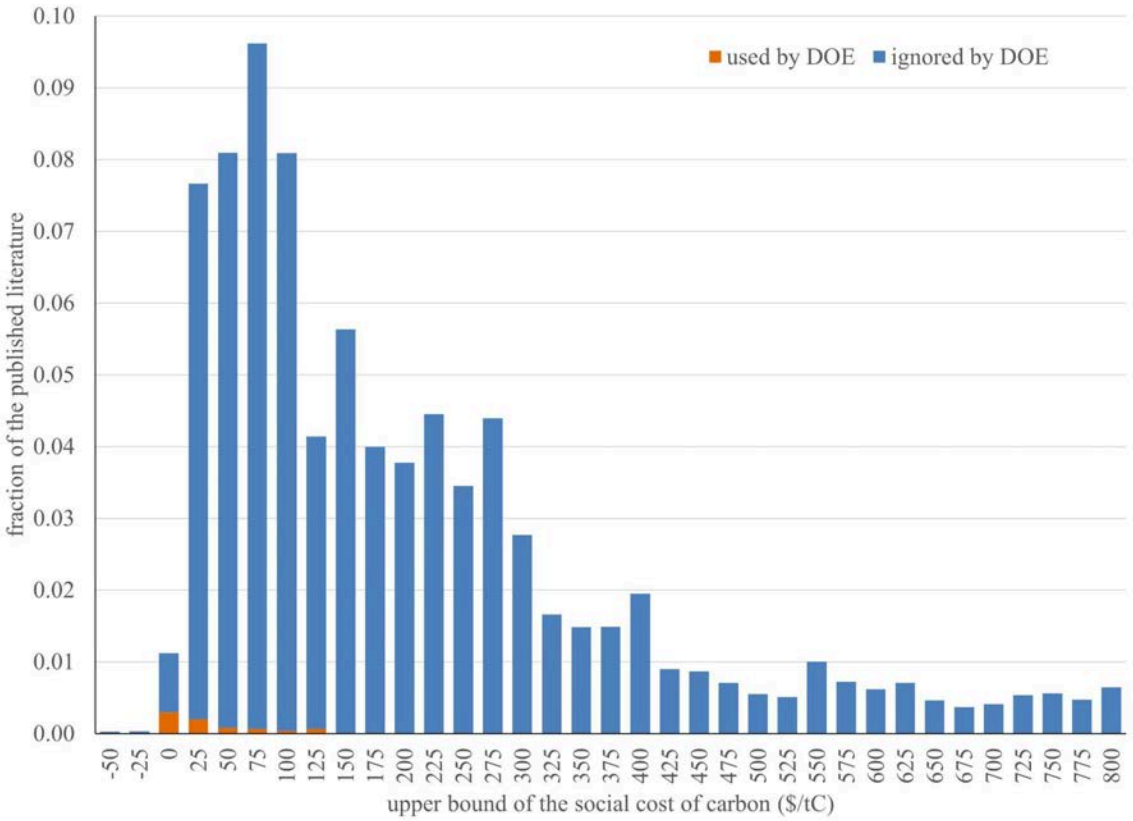
Overall assessment

This part of the response is a **technical** comment on Chapter 11 of the DOE CWG report (pp. 116–128). Before raising particular points about particular sections and paragraphs, we start with an overall assessment of Chapter 11.

Chapter 11 tries to argue that the risks of climate change to the U.S. economy are so small that almost no greenhouse gas emission reduction measures pass a cost-benefit test. The authors misrepresent the economic literature through selective citation and frequent miscitation, examples of which we describe below. However, the authors fail in what appears to be their mission: They conclude that climate change is likely to reduce economic output and welfare in the U.S.

Chapter 11 has several key omissions.

First, the authors ignore the risks and uncertainties about climate change and its impacts (Weitzman 2009; Nordhaus 2013; Berger, Emmerling, and Tavoni 2017; Gillingham et al. 2018; Y. Cai and Lontzek 2019; Barnett, Brock, and Hansen 2020; van den Bremer and van der Ploeg 2021; Lemoine 2021; Traeger 2023). They focus not so much on the central part of the distribution of possible outcomes, but zoom in on its left tail where everything turns out better than most analysts expect let alone fear (e.g., Figure 1). A long history of work on risk management points to the importance of considering the full distribution of outcomes, including tail risks (e.g., Heal and Millner 2014; Barnett, Brock, and Hansen 2020). The authors' general approach would also argue against the purchase of insurance, the maintenance of a standing army in peacetime, and many other regularly accepted precautionary measures (Weitzman 2012).



Estimates are classed into intervals of $25/tC; the number on the horizontal axis is the upper found of that interval; for example, the estimate marked "0" denotes estimates between -$25/tC and $0/tC. Estimates are for emissions in 2025 and given in 2024 U.S. dollar per metric tonne of carbon. Estimates are quality-weighted and censored as in Tol (2023). Data are from Tol (2024b).

Second, the authors limit their attention to the U.S., but do not defend their position. They ignore the implications of a breakdown of global cooperation on climate policy, they do not discuss the legal (Kuhn 1938) and moral (Kelleher 2025) implications of ignoring impacts outside the U.S.A., and they steer clear of the impacts of climate change on international trade (Brown et al. 2017; Baldos, Hertel, and Moore 2019; Dallman 2019), investment (Klusak et al. 2023), and migration (R. Cai et al. 2016; Mahajan and Yang 2020; Burzynski et al. 2022). As the National Academies (2017) noted, estimates of the domestic costs of climate change "need to consider the potential implications of climate impacts on, and actions by, other countries, which also have impacts on the United States."

The chapter also contains numerous errors and misrepresentations, as detailed in the comments below.

To correct the issues describe in this critique, the DOE CWG should:

- Evaluate the full distribution of the economic impacts of climate change, including the potential for significant downside risk and the role of precautionary measures.
- Summarize the full peer-reviewed literature on the national and subnational economic impacts of average and extreme weather.
- Incorporate a complete range of the literature's estimates of the SCC, including by following the recommendations of the National Academies (2017) when evaluating the diverse approaches to SCC calculations.
- Appropriately distinguish between unknown likelihood, high-impact outcomes and high impact outcomes known to be of low likelihood, and incorporate literature on decision-making under deep uncertainty when evaluating the impacts and implications of climate tipping points.
- Correct other misrepresentations identified in this critique.

Climate Change and Economic Growth

This section of the comment is a **technical** comment on section 11.1 of the DOE CWG report (pp. 116–120).

This section of the DOE CWG report discusses the impact of climate change on economic growth, arguing that "CO_2-induced warming might have some negative economic effects but they are too small to justify aggressive abatement policy." In making this argument, the CWG misrepresents the literature. For example:

- The DOE CWG writes:

  Mohaddes et al. (2023) found that warm weather shocks have small negative effects on U.S. state-level output but not income, while cold weather shocks negatively affect both and the impacts are about four times larger, implying a shift to warmer conditions would, if anything, yield a net economic benefit for the U.S. (p. 117)

In fact, Mohaddes et al. (2023) find that warm weather shocks have a significantly negative impact of unusually hot weather on income per capita in the last 33 years of the sample (1983–2016), but not in the whole sample (1963–2016). They find a negative impact on total output in the whole sample. This may mean that migration offset the economic impact before 1983 but not after. This study suggests that the U.S. economy has become more vulnerable to hot weather. The DOE's 'net benefit' would be

smaller if weather variability increased. The DOE ignores that Mohaddes et al. (2023) also finds significant impacts of precipitation shocks.

- The DOE CWG writes:

  economists have long been reluctant to endorse attempts to "stop" climate change or even aggressively reduce GHG emissions because the costs would not be worth it (p.117)

This twists a key message in the literature. Since Nordhaus (1982), economists have consistently argued for greenhouse gas emission reduction. See, for example, Akerlof et al. (2019), calling for a robust, gradually rising carbon tax-and-dividend system; the original signatories of this statement included 28 Nobel Memorial Prize laureates, 4 former Federal Reserve chairs, and 15 former Council of Economic Advisers chairs.

- The DOE CWG writes:

  [t]he Optimal Policy scenario [of Nordhaus (2018)] barely deviates from the business-as-usual path. (p.118)

This is a misrepresentation of the emissions trajectories shown in Figure 2 of Nordhaus (2018). Global fossil fuel CO_2 emissions in Nordhaus (2018)'s baseline scenario grow from about 35 Gt in 2015 to nearly 60 Gt in 2050 and over 70 Gt in 2100. Emissions in Nordhaus (2018)'s Optimal Policy scenario peak in 2045 at 40 Gt and decline by 2100 to about 15 Gt. DOE correctly notes that Nordhaus (2018) projects 3.5°C warming in his Optimal Policy scenario, but this reflects limitations of Nordhaus (2018)'s physical model. The calculated Optimal Policy CO_2 trajectory resembles that of the SSP2-4.5 emissions pathway, which has a median warming based on IPCC AR6 of about 2.7°C in 2100.

DOE dismisses without proper citation or substantive discussion the most recent version of Nordhaus's DICE model, DICE-2023 (Barrage and Nordhaus 2023) (p. 119). DICE-2023 includes a more realistic representation of the coupled climate and carbon cycle (following the guidance of the National Academies (2017)), as well as allowing for abatement of land use CO_2 emissions and non-CO_2 emissions. This improved physical representation leads to substantially more realistic temperature outcomes. For example, in DICE-2023, limiting warming to below 2°C requires net-zero CO_2 emissions by 2100; by contrast, Nordhaus (2018) erroneously calculated that limiting warming to 2.5°C would require zero fossil emissions by 2040.

- The DOE CWG writes:

  Greßer et al. (2021) developed a regional economic data set for 1,542 sub-national regions around the world between 2005 and 2015 and found temperature had no effect on growth. (p. 119)

Gresser, Meierrieks, and Stadelmann (2021) do not study the impact of climate on growth. They study its impact on output. DOE do not cite Callahan and Mankin (2022), Damania, Desbureaux, and Zaveri (2020), Kalkuhl and Wenz (2020), Kotz et al. (2021), Kotz et al. (2022) and Kotz et al. (2024) who use subnational data and find significant effects of weather on economic growth.

- Referring to Newell, Prest, and Sexton (2021), the DOE CWG writes:

  the net effect is likely positive but too uncertain to distinguish from zero (p. 119)

Figure 6 in Newell, Prest, and Sexton (2021) reveals that this holds for their "growth" models but not for their "level" models, which show a loss of several percent of GDP at the end of the century. Newell, Prest, and Sexton (2021) show that level models are more likely to cross-validate than growth models. DOE thus highlight the results of the poorly performing models and suppress the findings from the models with better forecast skill.

- The DOE CWG cites Berg, Curtis, and Mark (2023), a working paper subsequently published as Berg, Curtis, and Mark (2024).

Regressing country-level GDP on global-mean temperature, as Berg, Curtis, and Mark (2024) do, is a fairly novel methodological choice that demands close scrutiny. Nonetheless, a key finding of this study is that "Richer countries, in particular, such as the United States, tend to experience negative impulse responses of real GDP per capita growth to increases in global temperature."

- The DOE CWG writes:

  An influential study in 2012 [(Dell, Jones, and Olken 2012)] suggested that global warming would harm growth in poor countries but the finding has subsequently been found not to be robust (Tol 2024). Studies that take full account of modeling uncertainties either find no evidence of a negative effect on global growth from CO2 emissions or find poor countries as likely to benefit from it as rich countries. (p. 120)

Tol (2024a) pools the estimated coefficients from eight growth models. The pooled estimate is very close to Dell, Jones, and Olken (2012), which thus appears to be the most robust in the published literature.

Tol (2024a), the only cited study in this paragraph, finds a negative effect of climate change on economic growth. He also finds that the impact is larger in poorer countries, a result that goes back to Schelling (1992), who provided the opening quote in this section of the DOE CWG report, and indeed further Albala-Bertrand (1993; Blaikie et al. 2003; Kahn 2005).

This section in the DOE CWG report ends with a partial reproduction of a graph from CEA and OMB (2023). The reproduced graph does not include the legend or caption, making it challenging to interpret without referring to the original. The graph shows consistently negative impacts of additional climate change on the US economy across twelve peer-reviewed studies. Four of these studies use the econometric methods that are the subject of the DOE CWG report. Impact studies using other methods, the majority of the studies in CEA and OMB (2023) and indeed the literature, are ignored by the DOE CWG report. The DOE trivializes the negative impacts as “a few percent” and “negligible”. As a reference, a one percent loss of GDP in 2025 would be a cost of about $300 billion; “a few percent” could equally well be phrased as “more than a trillion dollars” (relative to today’s economy). Further, referring to the economic impacts of climate change as “negligible” directly contradicts the text of the CEA-OMB report, which states that “[c]limate change presents many economic, financial, and fiscal risks across the United States,” and that “[e]ven under the low global emissions scenario, the consequences of physical climate damages for the macroeconomy weaken the fiscal outlook” (CEA and OMB 2023).

Models of the Social Cost of Carbon

This section of our comment is a **technical** comment on section 11.2.1–11.2.2 of the DOE CWG report (pp. 120–122).

This section of the DOE’s report discusses the estimation of the social cost of carbon (SCC), or the net present value of the monetized welfare loss associated with an additional ton of CO_2 emitted. The first few paragraphs of this section do correctly summarize various methods used to estimate the SCC. However, the overall conclusions of the report are out of step with the consensus of the literature on the SCC. For example, the National Academies undertook an extensive study of

approaches used to estimate the SCC (National Academies 2017), prompting multiple research teams to engage in multi-year efforts to update the SCC with new damage functions, socioeconomic scenarios, climate modeling, and approaches to risk and discounting (EPA, 2023). Further, multiple recent meta-analyses have synthesized the state of the literature on the SCC, yielding consensus values far higher than the DOE CWG's perspective. Tol (2023) showed that SCC estimates have risen over time, and Moore et al. (2024) showed that experts judge the true SCC to be even higher than recent government updates, approximately \$283 tCO_2^{-1} when combining expert judgement with published estimates. These syntheses of the literature account for the myriad potential variations in SCC estimates, and yet are not discussed by the DOE CWG report.

Additionally, the authors take it as given that the appropriate discount rate is high. They do not defend this position, which is at odds with the view of many economists (Weitzman 2001; Freeman and Groom 2015; Drupp et al. 2018; Bauer and Rudebusch 2023; Nesje et al. 2023).

Many of the other parts of this chapter rely on critiques of mainstream equilibrium climate sensitivity (ECS) estimates and on revised estimates of the agricultural impacts of global warming. These critiques are discussed in more detail in Chapters 4 and 9 of the DOE CWG report, respectively, and we refer the reader to the corresponding sections of this reply.

This section also contains a variety of errors.

- Some errors suggest carelessness. For example, the DOE CWG writes

  in DICE only three were needed and were originally chosen to assign a predetermined penalty (1.2%) to global output as of 3 warming

In fact, Nordhaus (1992) assumed 1.33%.

- Other errors suggest misunderstanding of how IAMs are calibrated. For example, the DOE CWG writes

  Every IAM assumes a value of the Equilibrium Climate Sensitivity (ECS), which controls the amount of warming that results from CO2 emissions and can be freely varied for the purpose of generating a distribution of SCC values associated with uncertainties over ECS. But as Roe and Baumann (2013) pointed out, time-to-equilibrium increases with the square of ECS, so an upward

adjustment of the ECS parameter without an appropriate slowing down of the adjustment process can yield distorted present value damage estimates. (p. 121)

This paragraph asserts that parameters in IAMs are singly rather than jointly calibrated. No evidence is provided. Specifically, following the recommendations of the National Academies (2017), the most recent U.S.-government-endorsed updates to the SCC (EPA, 2023) embed reduced-complexity climate models into their IAMs, which numerically integrate the climate response to CO_2 perturbations accounting for deep-ocean adjustment times similar to the recommendations by Roe and Bauman (2013). They do not simply specify an ECS value.

- The section also makes assertions regarding the content of the EPA SCC's report that do not correspond to the report. The DOE CWG writes

  For example, in 2023 the U.S. Environmental Protection Agency raised its preferred SCC value about 5-fold over the estimates it had issued ten years earlier. This is not because new data had been collected or better mathematical methods had been invented, but because new assumptions had been used, and the validity of those assumptions was a separate question. Tabulations on EPA (2023) p. 81 show that if assumptions similar to earlier analyses had been applied, the results would not have materially differed from before. (p. 122)

No such table appears in the EPA (2023) report. The table on page 81, as cited by DOE, disaggregates the SCC into categories of damages (e.g., agriculture vs. sea level rise) but makes no claim about assumptions relative to earlier analyses. Further, no other table in the EPA SCC update makes such a claim. Indeed, Table 5.1 of EPA (2023) shows a range of updates made by the EPA in their SCC estimation that go beyond "new assumptions", including new probabilistic socioeconomic scenarios, new carbon-climate modeling, and updated sectoral damage functions based on peer-reviewed literature.

The distribution of social cost of carbon estimates

This section of our comment is a **technical** comment on section 11.2.3 of the DOE CWG report ("Evidence for low SCC," p. 123).

- The DOE CWG present an unbalanced overview of the distribution of SCC estimates. The DOE CWG writes

> *Dayaratna et al. (2023) used the updated empirical ECS distribution estimate of Lewis (2022), which assimilates modern instrumental and paleoclimatic temperature records, and allowed for a 30 percent gain in the CO2 fertilization benefit in the FUND model, and found that, even at a low discount rate of 2.5 percent, the median SCC as of 2050 is only $18.67, with a 24 percent probability of the true value being negative. At a five percent discount rate the median SCC value is negative until the mid-2040s and at 2050 was only $0.37 with a 49 percent probability of being negative. Thus, under reasonable assumptions a mainstream IAM using updated scientific inputs yields evidence consistent with the SCC not being significantly greater than zero. (p. 123)*

Dayaratna and McKitrick (2023) find a mix of social costs and social benefits of carbon, depending on the discount rate used, the assumed distribution of the climate sensitivity, and the assumptions on the strength of CO2 fertilization. DOE omits the social cost, even though the average social cost of carbon in Dayaratna, McKitrick, and Kreutzer (2017), Dayaratna, McKitrick, and Michaels (2020), and K. D. Dayaratna and McKitrick (2023) is a positive $24/tC.

Using FUND, the same model as Dayaratna and McKitrick (2023), Anthoff, Tol, and Yohe (2009) similarly found that CO2 fertilization may — in the near term, for certain parameter values, and considering only the impacts represented in FUND — offset the negative impacts of a marginal increase in climate change. They noted, however, that this effect is less important in developed economies such as the U.S.A., and moreover disappears when parameter uncertainty is accounted for. Anthoff, Tol, and Yohe (2009) further did not benefit from the last 16 years of modeling and empirical studies of climate change impacts on agriculture, discussed elsewhere in this review, as well as subsequent studies on other major impacts like mortality. DOE does not refer to Anthoff, Tol, and Yohe (2009), even though it directly contradicts its conclusions.

Where Dayaratna, McKitrick, and Kreutzer (2017) and Dayaratna, McKitrick, and Michaels (2020) use the mean social cost of carbon, Dayaratna and McKitrick (2023) use the median. The distribution is right-skewed, so the median is smaller than the mean. The latter is the appropriate statistic, as the SCC should represent the expected (i.e., mean) welfare change in response to a marginal emission.

Figure 1 compares the histogram of estimates of the social cost of carbon by Dayaratna and colleagues to the wider literature. DOE emphasizes a small and unrepresentative part of the literature.

- In arguing that the SCC should be balanced against the private marginal benefits of fossil fuel use, the DOE CWG writes

  Tol (2017) estimates that the private benefit of carbon is large relative to the social cost. (p. 123)

Tol (2017) was peer-reviewed and rejected because it compares the average private benefit to the marginal social cost. DOE faithfully reproduces this error.

Tipping Points

This section of our comment is a **technical** comment on section 11.2.4 of the DOE CWG report (pp. 123–125).

The DOE CWG report presents a confused interpretation of tipping points and nonlinear behaviour in the climate system. The authors attempt to draw a distinction between abrupt changes instigated by forcing and the existence of bifurcations/bistability in a natural system. This distinction is erroneous. An abrupt transition, whether or not it is initiated by forcing or natural variability, is indicative of a system's bistability (Ashwin et al., 2013). A system that responds linearly to applied forcing (as in the case of summer Arctic sea ice cover) is not considered to exhibit tipping behaviour either by the IPCC (see below) or by the wider scientific community.

The author's errors here may be unintentional, as there is considerable diversity in the use of the phrase "tipping point" in the literature (Kopp et al., 2025). However, the DOE CWG leverages this ambiguity to make more stark and consequential errors in their subsequent discussion.

The authors imply in particular (p124) that because bifurcations are a "natural" feature of the climate system, an abrupt transition between states would be just as likely under small perturbations as they would be under forcing. They use this framing to suggest that such transitions may occur irrespective of the forcing and could be inherently unpredictable. This framing is misleading, and is inconsistent with the published literature (Ashwin et al., 2013; Dijkstra and Ghil, 2005; Chapman et al., 2025). While systems can "tip" through small perturbations (noise), the *likelihood* of their tipping is increased by forcing (when the forcing is acting to move the system towards its bifurcation point). It is for this reason that possible ‘tipping points’ in the climate system (such as the Atlantic Meridional Overturning Circulation, AMOC) are generally

considered *both* challenging to predict *and* plausibly more likely to occur under (GHG) forcing. This is a point that the authors seem to intentionally obfuscate.

The further assertion (p124) that "AR6 (WGI, Chapter 1) focuses mainly on the first type of tipping point" is incorrect both for the reason noted above (there is no real distinction between the first and second "types" of tipping) and from the IPCC's own definition: (Chen et al., 2021):

> *"In some cases, abrupt change occurs because the system state actually becomes unstable, such that the subsequent rate of change is independent of the forcing. We refer to this class of abrupt change as a 'tipping point', defined as a critical threshold beyond which a system reorganizes, often abruptly and/or irreversibly"*

A system evolving "independent of the forcing" to be "reorganized" is a nonlinear transition between stable states, i.e. a bifurcation. Indeed, prominently consequential --- and most widely considered --- tipping points (e.g. AMOC, Greenland and West Antarctic Ice Sheets) are classic examples of system bistability and bifurcations. In these systems there are suspected but poorly quantified feedbacks that accelerate changes as they emerge (e.g. salinity feedbacks in the AMOC, and marine ice sheet instability in West Antarctica), meaning that the systems can transition abruptly between quasi-steady states (an on-off state of the AMOC, a marine-terminating or land based ice sheet) with distinct timescales for reversal.

The statement that "there seems to be very little to discriminate between a tipping event and natural climate variability" is false. It is helpful here to discriminate between the variability that a system exhibits on some timescale of interest (sometimes called intrinsic or natural variability; e.g., ENSO for interannual timescales) and the *possibility* of a system – through its inherent or natural dynamics – to exhibit much more significant and sustained changes on a comparable timescale. The occurrence of such an abrupt change/tipping event is widely disambiguated from "natural climate variability" as being (a) much larger in magnitude than the intrinsic variability that occurs on comparable timescales and (b) sustained on timescales longer than that of the intrinsic variability. For example, while the AMOC varies by ±30% from year to year (intrinsic variability; McCarthy et al., 2025), a characteristic tipping event could see it decline by 70-100% and remain at this low level for decades (van Westen et al., 2024). Consistent with the rest of the section, the authors confuse a system's chaotic behaviour with its bistability; and obfuscate the meaning of the term "natural climate variability".

Below, we refute many of the more specific claims of the DOE CWG report, particularly in their assertions about the conclusions and assigned uncertainties in the IPCC AR6.

The DOE CWG writes:

> *Natural climate variability has in the past produced shifts in the West African and Indian monsoons, Amazon forest and coral reef dieback, and disintegration of parts of the Greenland and West Antarctic ice sheets. These impacts can reverse on the decadal and century timescales associated with natural climate variability and ecosystem responses. (p. 124)*

While monsoonal shifts can indeed occur on decadal timescales, ice sheets take many millennia to regrow. Changes in the polar ice sheets triggered by human-caused warming will not rapidly be reversed.

Regarding the Atlantic Meridional Overturning Circulation (AMOC) and the West Antarctic Ice Sheet (WAIS) in particular, the DOE CWG quotes selectively from the IPCC AR6 Summary for Policy Makers to support the statement that “the IPCC assigns low probabilities to most, including the largest ones” (p. 125).

In fact, regarding AMOC and WAIS, AR6 concludes that the likelihood of collapse is poorly known, not that it is known to be low. This situation is quite distinct from that of a “large meteor strike” (p. 124), where the statistics of objects that might collide with the Earth can be measured and thus the likelihood of a large strike objectively determined. A better analogy would be the possibility of a world war, about which different experts may have fundamentally different mental models and consequently different assessments of likelihood.

DOE is perhaps confused by the IPCC’s description of rapid ice sheet loss as a “low-likelihood, high-impact outcome.” This results from a confusing definition of the phrase “low-likelihood, high-impact”, as flagged in Kopp et al. (2023) and Lempert et al. (2024). IPCC (2021) writes: “Low-likelihood, high-impact” outcomes are defined as outcomes “whose probability of occurrence is low or *not well known (as in the context of deep uncertainty)* but whose potential impacts on society and ecosystems could be high.” (Emphasis added)

Regarding AMOC, AR6 (Fox-Kemper et al. 2021) notes: “None of the CMIP6 models features an abrupt AMOC collapse in the 21st century, but they neglect meltwater release from the Greenland Ice Sheet. Also, a recent process study reveals that a collapse of AMOC can be induced, even by small-amplitude changes in freshwater

forcing (Lohmann and Ditlevsen, 2021). As a result, we change the assessment of an abrupt collapse before 2100 to *medium confidence* that it will not occur."

Note that AR6 does not say that AMOC collapse *in general* is of low likelihood, but that there is *medium confidence* (in IPCC terminology, a medium amount of evidence with a medium amount of agreement of evidence) that it will not occur *in the 21st century*. This assessment is weaker than prior assessments, which found that an AMOC collapse in the 21st century was *very unlikely* – evidence published in the years since the Fifth Assessment Report in 2013 added more ambiguity to the assessment of AMOC collapse. Further, evidence is insufficient to support an assessment of the likelihood of collapse beyond the 21st century.

Regarding high-end sea level outcomes driven by rapid ice-sheet mass loss, AR6 (Fox-Kemper et al. 2021) writes:

"Because of our limited understanding of the rate at which some of the governing processes contribute to long-term sea level rise, we cannot currently robustly quantify the likelihood with which they can cause higher sea level rise before 2100 (Stammer et al., 2019).... The [high-end sea level] storyline considers faster-than-projected disintegration of marine ice shelves and the abrupt, widespread onset of marine ice cliff instability (MICI) and marine ice sheet instability (MISI) in Antarctica (Section 9.4.2.4), and faster-than-projected changes in both the surface mass balance and dynamical ice loss in Greenland. While conceptual studies provide medium evidence of these processes, substantial uncertainties and low agreement in quantifying their future evolution arise from limited process understanding, limited availability of evaluation data, missing or crude representation in model simulations, their high sensitivity to uncertain boundary conditions and parameters, and/or uncertain atmosphere and ocean forcing (Sections 9.4.1.2; 9.4.2.2)"

Note that AR6 does not assess these processes as being of low likelihood, but as being not possible to robustly quantify.

Like ice sheet collapse and post-2100 AMOC collapse, many other so-called 'tipping points' are of similarly poorly known likelihood, though it is possible to put some constraints on their likelihood at different levels of warming, and in general their likelihood does indeed increase as human activity pushes the Earth system further and further from its historical state (Armstrong McKay et al. 2022). DOE should accordingly clarify that, for many potential abrupt changes, IPCC does not "assign[] low probabilities," but rather assesses the likelihood to be poorly known, but increasing with global warming.

Notably, an extensive literature addresses decision-making under conditions of deep uncertainty (e.g., Lempert et al. 2024), as in the case of these 'tipping points' with poorly known likelihoods. In general, the appropriate response is not to treat such outcomes as of zero expected cost. Such deep uncertainties do pose a challenge to the use of benefit-cost analysis to support decision making (Keller, Helgeson, and Srikrishnan 2021). This literature is not addressed in the report.

Are there alternatives to the social cost of carbon?

This section of our comment is a **technical** comment on section 11.2.5 of the DOE CWG report (p. 125).

This brief report section addresses the question of whether estimates of the SCC are useful for policymakers. The value of uncertain social cost estimates is a long running discussion in economics; as one example, see Baumol (1972). This literature is not referenced.

This section confuses the SCC as a 'regulatory instrument' (presumably referring to its use as a metric in regulatory impact analyses) with the SCC as a metric of marginal damages caused by a tonne of carbon dioxide. The latter is clearly an appropriate topic for a report on "impacts of greenhouse gas emissions." The former is about policy choices, not a description of impacts, and seems beyond scope. Accordingly, section 11.2.5 should be removed. If it is not removed, the broad base of relevant literature needs to be reviewed.

The statement that "Many emissions regulations (such as electric vehicle mandates, renewable energy mandates, energy efficiency regulations and bans on certain types of home appliances) cost far more per tonne of abatement than any mainstream SCC estimate" is an uncited assertion without evidence. Of course, as discussed above, the report's definition of 'mainstream' is idiosyncratic, as it likely refers to the left tail of SCC estimates highlighted in red in Figure 1 and leaves the bulk of SCC estimates, not discussed in the report, to be 'not mainstream.' This statement should be removed.

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Response to Chapter 12: Global climate impacts of U.S. emissions policies

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This final chapter of the DOE CWG report, beginning on **page 129**, and concluding on **page 131**, makes the case that U.S. policy action on climate mitigation will have a minimal effect on the global climate, and that these effects will only take place after a long period of time: "U.S. policy actions are expected to have undetectably small direct impacts on the global climate and any effects will emerge only with long delays."

The framing of this chapter is built upon several logical fallacies about the US contribution to global climate change, and upon premises which are inconsistent with longstanding established scientific and institutional understandings of the need for collective action, and the responsibilities of all nations in cooperating on climate change mitigation. This chapter also makes multiple false statements about current scientific understanding in order to minimize the impact of CO2 in the immediate future and at the local level.

Global climate change is understood to be a "global commons problem" due to the long lifetimes of the main greenhouse gases and the global response of the climate system. As such, emissions from one location today affect climate in ways that are distributed globally and in time into the future. The DOE CWG report implies that even large actions to reduce US GHG emissions will cause small climate reduction benefits to the US itself. However, that statement would also be true for every other nation considering mitigation. For a commons problem of this nature, collective action where many nations all take action is essential to achieve a better global solution (Ostrom 1990).

Response to 12.1:

Comment: Technical, Editorial

First, the DOE CWG report claims that there is a "scale problem" such that U.S. emissions have minimal global impact, stating that "even drastic local actions will have negligible local effects" (p.129). This is a misrepresentation of both present and historical data on U.S. emissions of greenhouse gases which have had, and continue to have, a substantial effect on the global climate to date, in absolute terms and relative to other nations.

- U.S. annual fossil emissions of CO2 (territorial) in 2023 (the latest year for which the Global Carbon Project has reliable data) show the United States to have *the second largest emissions of all countries that year* (at 4,911.4 million tonnes of *CO2*). This equates to 13% of World emissions of *CO2* that year. This means the U.S. emits more *CO2* annually than all other countries, with the sole exception of China (Global Carbon Project 2024).
- Data from the EDGAR database on total greenhouse gas emissions, which includes CO2, methane, and other greenhouse gases, similarly shows the outsized footprint of U.S. emissions. In 2023, the U.S. was the *second-largest emitter* with 5,960.8 million tonnes of CO2e, or 11% of the world total (Crippa et al. 2024).
- Cumulative U.S. fossil emissions of *CO2* (territorial), from 1850 to 2023, show that the U.S. has emitted 431,683 million tonnes of *CO2* during this period. This is *more than any other country's cumulative fossil CO_2 emissions*, and equivalent to 23.9% of World fossil CO2 emissions over this time (Global Carbon Project 2024)
- According to Ritchie et al. (2023), cumulative U.S. CO_2 emissions from fossil fuels, cement, land use, and land use change and forestry combined, from 1850 to 2023, show the US is the largest emitter of all countries, at 559.32 billion tonnes CO_2. This is equivalent to 21.3% of World cumulative anthropogenic CO_2 emissions during this same period.Therefore the US has likely contributed more than any other country to ambient CO_2 concentrations and warming experienced to date.
- As per the IPCC (2021), anthropogenic CO_2 emissions from 1750 through to 2019 have contributed to an estimated change in Global Mean Surface Temperature of roughly 0.95°C. The vast majority (more than 99.9%) of cumulative anthropogenic CO_2 emissions occurred after 1850 (Ritchie et al. 2023). Based on the additionality of cumulative CO_2 emissions, it is reasonable to conclude that the U.S. is responsible for an equivalent (~21.3%) amount of the warming that has occurred so far (see Evans 2021 for similar estimates).
- Putting historical emissions aside, the DOE CWG report frames U.S. emissions as relatively small in a way that downplays the significance of current and future emissions without regulation. To properly assess the impact of U.S. climate policy, however, one cannot look only at a single year's emissions. What matters is how regulation alters the trajectory of emissions in future years, since the climate impacts of CO_2 depend on *cumulative emissions*.
- The premise of the United Nations Framework Convention on Climate Change (UNFCCC), which the U.S. signed and ratified in 1992 – and to which the US is still a party – acknowledges that climate change is a collective problem requiring action on behalf of *all nations*, as no single country can achieve the goals of the UNFCCC on its

own. It states on the first page, that "the global nature of climate change calls for the widest possible cooperation by all countries and their participation in an effective and appropriate international response, in accordance with their common but differentiated responsibilities and respective capabilities and their social and economic conditions" (United Nations 1992, 1).

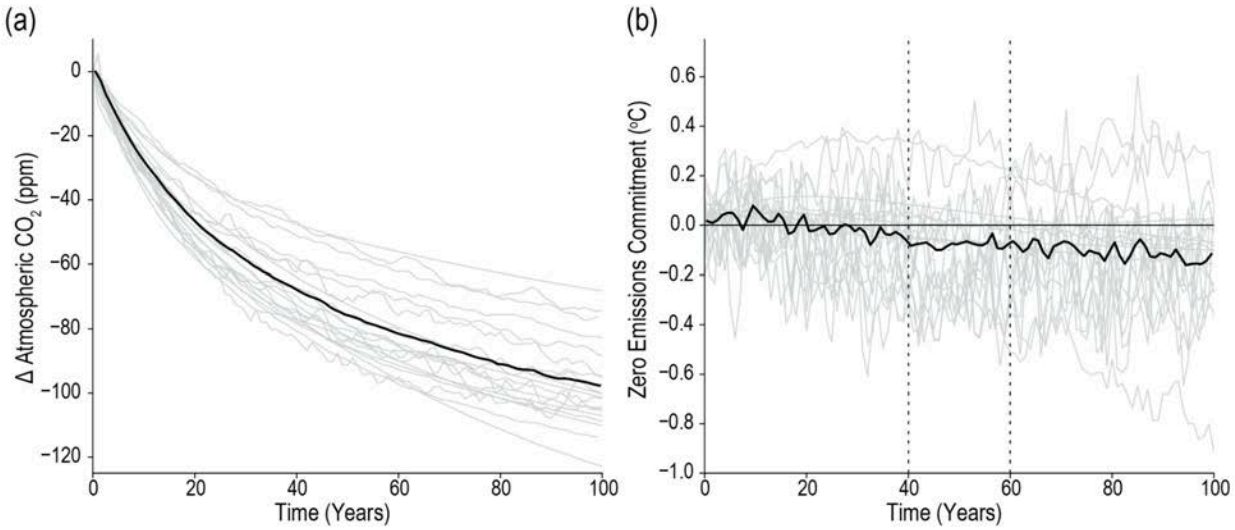
    - Clause 2.a (p.6) of the UNFCCC asserts that "Parties shall adopt national policies and take corresponding measures on the mitigation of climate change, by limiting its anthropogenic emissions of greenhouse gases and protecting and enhancing its greenhouse gas sinks and reservoirs". Further, the Convention asserts that "developed countries are taking the lead in modifying longer-term trends in anthropogenic emissions" (p.6) – including specifically emissions of carbon dioxide (United Nations 1992). It thus stands to reason that under the US' obligations to the UNFCCC, further action to limit anthropogenic emissions of CO_2 is required.
    - As a party to the UNFCCC, the U.S. further agreed that "States have … the responsibility to ensure that activities within their jurisdiction or control do not cause damage to the environment of other States or of areas beyond the limits of national jurisdiction" (p. 2).
    - Given that the US contribution to radiative forcing from CO_2 to date is objectively "large" given its status as the highest-emitting cumulative country (at nearly one fifth) of cumulative CO_2, it stands to reason that the U.S. bears a proportionally significant responsibility to lead in emissions reductions under the principles of the UNFCCC. This further illustrates the misrepresentative assertion in the DOE CWG of "only a very small global effect" (p. 129).

- To address these issues, the DOE CWG would need to:
    - Revise its characterization of the "small" scale of U.S. emissions, and provide context of U.S. emissions relative to other countries.
    - Reframe U.S. emissions in terms of more than just one individual year, as that is not a scientifically rigorous or politically relevant method of accounting.
    - Discuss the role of the U.S in the broader global community and of climate change as a global commons collective action problem, and the role of common but differentiated responsibilities of nations in collectively mitigating climate change.

Comment: Technical

Second, the DOE CWG falsely states "And even if global emissions were to stop tomorrow, it would take decades or centuries to see a meaningful reduction in the global CO2 concentration and hence human influences on the climate."

- Stopping all CO2 emissions would lead to a meaningful reduction in global CO2 concentrations, as is shown in the figure below. Surface warming stays level because it is a balance between the radiative forcing due to CO2 in the atmosphere and energy take-up by the ocean. Stopping global emissions tomorrow would lead to an immediate decline in CO2 concentration, one that is actually more rapid in the years immediately after cessation. From research in the zero emissions commitment model intercomparison project (ZECMIP) (Jones et al. 2019): "By 50 years after emissions cease in the A1 experiment, the change in atmospheric CO2 concentration ranged from −91 to −52 ppm, with a mean of −76 ppm and median of −80 ppm," where A1 is a 1% increase in CO2 until 1000 PgC of emissions are reached, and then emissions are zeroed (MacDougall et al. 2020).
- This is also discussed in the IPCC AR6 WG1 report, as seen in Figure 4-39 below. In line with the work discussed above, this figure is showing the response to a cessation of CO2 shows the immediate decline in CO2 concentrations and that shows that warming stops basically immediately when emissions cease.



*From Fig. 4-39 of the IPCC AR6 WG1 report: "**Figure 4. 39 | Zero emissions commitment (ZEC).** Changes in **(a)** atmospheric CO_2 concentration and **(b)** evolution of global surface air temperature (GSAT) following cessation of CO_2 emissions branched from the 1% per year experiment after emissions of 1000 Pg C (Jones et al., 2019; MacDougall et al., 2020). ZEC is the temperature anomaly relative to the estimated temperature at the year of cessation. ZEC_{50} is the 20-year mean GSAT change centred on 50 years after the time of cessation (see Table 4.8) –*

this period is marked with the vertical dotted lines. Multi-model mean is shown as a thick black line, individual model simulations are in grey. Further details on data sources and processing are available in the chapter data table (Table 4.SM.1)."

Comment: Technical, Editorial

Third, on pages 129-131, DOE CWG frames U.S. policy action as if it exists within a global policy vacuum, having no relative effect on the actions of other nations or the international community. In contrast, numerous lines of evidence suggest that U.S. policy action has spillover effects on the global community.

- A report by Elkerbout et al. (2024) shows how the U.S. Inflation Reduction Act (IRA) catalyzed European climate policy shifts, noting that "the Net-Zero Industry Act (NZIA) was adopted by the EU's Council of Ministers on May 27, 2024, and has been referred to as the European Union's response to the US Inflation Reduction Act." (p. 4). The authors find that the EU's climate policy shifted after the release of the IRA, from primarily supporting early-stage innovation to prioritizing the deployment and market penetration of strategic net-zero technologies, a clear strategic pivot which increased policy ambition.
- Similarly, work by Mehling (2024) and Larsen et al. (2023) has contributed to a broader literature pointing to potential spillover effects of US climate policy in other countries. As Mehling summarizes:
    - "Research suggests that the IRA could not only help significantly reduce the distance between forecast emissions levels and the U.S. Nationally Determined Contribution (Bistline et al. 2023; Jenkins et al. 2023) while already evidencing signs of a resurgence in U.S. manufacturing (Bermel et al. 2023), but it is also expected to lower the costs of clean technologies, benefiting other countries through knowledge diffusion and technology transfer, and over time unlocking greater deployment of clean technologies outside the U.S. than within its borders" (Mehling 2024, 24).
    - Similarly, Mehling adds, in reference to Larsen et al. (2023), that "one study has estimated that, on a cumulative basis, the incentives included in the IRA for certain emerging climate technologies could reduce 2.4-2.9 tons of emissions outside the U.S. for every ton reduced within the U.S."
- Broadly, there is an extensive literature on policy diffusion that shows how the adoption of a policy in one jurisdiction spreads by influencing the policy choices of other governments (see Shipan and Volden 2012 for a review). This extends to climate policy

as well, in the contexts of carbon pricing (Linsenmeier et al. 2023) and industrial policy (Meckling and Strecker 2023).

- It thus stands to reason that ambitious climate policies in the United States are likely to generate positive spillover effects – encouraging greater action and ambition abroad. Conversely, a rollback of U.S. climate efforts could produce *negative* spillovers, potentially weakening international resolve and slowing the pace of global decarbonization.
- To address these issues the DOE CWG would need to:
    - Incorporate recent research that clearly shows the influence of U.S. regulation and policy on more than just the U.S., and how that type of spillover has carried through with climate policy and regulation, thus making the clear tie that U.S. choices impact more than the U.S.

Comment: Technical, Reference

Fourth, DOE CWG falsely states that CO2 has a time-delayed impact: "Any change in local CO2 emissions today will have only a very small global effect, and only with a long delay" (p.129):

- The claim that there is a "long delay" before effects are felt is incorrect. Immediately after emissions, CO2 has an impact on radiative forcing in the atmosphere that then lasts for hundreds to thousands of years, due to the long lifetime of CO2. The authors are making a misleading argument, confusing long lifetimes with long delays in impact. The impact is almost immediate, with warming stopping immediately after emission cessation, and concentrations immediately declining (see Figure 4-39 from the IPCC AR6 WG1 report, above)
- This combination– one in which CO2 emissions have an *immediate* impact on radiative forcing, while also having a *long* lifetime, actually means that their impact is not only *immediate* but also *long lasting* and can accumulate over long time periods.
- Although closing one individual coal plant, or changing one combustion vehicle to an electric vehicle will have a small impact, U.S. policy has much wider reach, impacting entire sectors that total to 4.7 gigatons of CO_2 emissions *annually* (6 gigatons of CO_2 equivalent) (U.S. Energy Information Administration 2025). Beyond this, U.S. policy impacts other countries and future time periods, leading to a compounding impact on global CO2 emissions.
- Thus, if we combine the logic of U.S. emissions compounding year on year, with the fact that CO2 does have an immediate and long lasting impact, this leads to an opposing conclusion to one that the authors draw.

- To address these problems, the DOE CWG would need to:.
    - Correct their clearly false statement that CO2 does not have an immediate impact, as upon being emitted it impacts radiative forcing and thus temperatures.
    - Discuss the long lifetime of CO2, and thus the importance of cumulative impacts of year on year emissions of CO2 given its immediate impacts.
    - Remove language on a long delay in CO2 impacts, given that this is untrue.

Comment: Technical, Editorial

Fifth, DOE CWG implies that US policy actions going forward will have a limited impact: "Any change in local CO2 emissions today will have only a very small global effect, and only with a long delay" (p.129). This is demonstrably false:

- According to a November 2024 analysis by Climate Action Tracker, current policies and actions in the U.S. put the country on track for a reduction of roughly 29%–39% below 2005 levels by 2030, *excluding land use, land use change, and forestry (LULUCF)*. This represents a shortfall in emissions reductions required to meet the U.S. target under the Nationally Determined Contribution of 50-52% below 2005 levels by 2030 (Climate Action Tracker 2024). The analysis rates this level of action as "insufficient", meaning that if all countries were to follow a similar scale of emissions change, global warming would likely exceed 2°C and could reach up to 3°C by the end of the century. Meanwhile, if the U.S. were to increase policy ambition and successfully bring emissions down by at least 62%–65% below 2005 levels by 2030 (including emissions from land use, land use change and forestry, or LULUCF), this level of action would be compatible with a 1.5°C warming trajectory (if all other nations pursued the same level of action) (Climate Action Tracker 2024).
- According to the Global Carbon Project 2024, the remaining carbon budget to limit global warming to 2.0°C is 1,110 Gt of CO2 (Global Carbon Project 2024). A simple comparative exercise for two distinct emissions pathways demonstrates how the DOE CWG report's claim that "any change in local CO2 emissions today will have only a very small global effect" (p. 129) is demonstrably false: Consider, for instance, the difference between an emissions trajectory in which U.S. fossil CO2 emissions remain at present day levels (roughly 4.9 Gt CO2 per year) for the remainder of the 21st Century (75 years, for illustrative purposes), as compared to a scenario in which U.S. fossil CO2 emissions decline to 'Net Zero' by 2050 (in 25 years). In the former case, cumulative CO2 emissions by end of Century are 367.5 Gt CO2, eating up *a third* of the global remaining

CO2 budget, and leaving all of the rest of the international community to the remaining *two thirds* of the budget. In the second Net Zero scenario, U.S. cumulative fossil emissions might feasibly total roughly 64 Gt CO2 by end of the century (or less than 6% of the remaining budget). In this scenario, the U.S.' more ambitious CO2 reduction leaves much more space for other nations to use a fair share of the remaining budget for 2.0°C. As such, it can be fairly stated that U.S. action has a substantial effect on the international community's ability to remain within the global carbon budget.

- To address these comments the DOE CWG needs to:
    - Show how different future emissions trajectories impact cumulative emissions outcomes by the end of this century, and how these different emissions scenarios impact the remaining carbon budget for 2.0°C warming of pre-Industrial levels.

Comment: Reference, Technical

Sixth, DOE CWG mischaracterizes and misuses evidence from Lomborg (2016) in quoting that the study "estimated that full compliance with the initial commitments in the Paris Accord would not stop warming, it would only prevent about 0.1C warming and delay hitting the baseline year 2100 temperature levels by about a decade" (p. 129).

- We assume the DOE CWG are discussing the emissions from the entire world, given that the authors claim 'full compliance… would not stop warming'. However, these are the 'initial commitments', as Lomborg (2016) himself states. His 'optimistic' scenario holds a reduction of 28% of CO2 emissions by 2025 until 2100, which is not in line with current policies and goals from the United States, as the current Nationally Determined Contribution that reflects updated U.S. policies is to reduce greenhouse gas emissions by 50-52% below 2005 levels by 2030 and 61-66% by 2035 (United States of America 2021). This use of emission reduction estimates that are far below the current commitments contribute to a large underestimate of the impact of the Paris Accord and the United States' NDCs.
- Beyond this, it is known that the current NDCs are not even enough to meet the ultimate Paris Accord goals. So the United States has a smaller impact that is necessary to meet the goals of the accord. Thus, the DOE CWG report's line of argument relies on the US failing to meet its commitment in the Paris Accord and only following old policies, rather than the impact of meeting the Paris Accord.

- Based on work that includes both NDCs and INDCs, the expected temperature increase due to current policies is 2.7°C to 3.2°C by 2100 (Rogelj et al. 2016; Hausfather 2025).
- To address this issue, the DOE CWG would need to:
    - Incorporate the most up-to-date literature on the impacts of current U.S. climate policy, as well as scenarios in which U.S. policy aligns with the Paris Accord. Relying instead on outdated policy assumptions that fall well short of Paris objectives results in a clear underestimate of the U.S.'s impact.

Comment: Technical, Editorial

Seventh, the DOE CWG states: "Thus, in contrast with conventional air pollution control, even drastic local actions will have negligible local effects, and only with a long delay. The practice of referring to unilateral U.S. reductions as 'combatting climate change' or 'taking action on climate' on the assumption we can stop climate change therefore reflects a profound misunderstanding of the scale of the issue." (p.129).

- As discussed above, the statement of a 'long delay' is false.
- Again, as discussed above, this statement assumes that the United States' actions do not impact other countries, when in fact, they do (Mehling 2024; Larsen et al. 2023).
- The statement of 'negligible local effects' is incorrect. The United States' emissions do impact the global mean temperature, and, thus, local temperature. The Fifth National Climate Assessment (Crimmins et al. 2023, 1–5): "The effects of human-caused climate change are already far-reaching and worsening across every region of the United States. Rapidly reducing greenhouse gas emissions can limit future warming and associated increases in many risks."
- Conventional air pollution has both *immediate* and *long-term impacts*, and much of the regulation surrounding air pollution is meant to reduce these long-term impacts from continued inhalation of polluted air (Pope 2000; McDuffie et al. 2021).
- Furthermore, air pollution is not just local– a critical issue regarding air quality is the transboundary and trans-continental spread of pollutants, as is addressed through the Convention on Long Range Transport of Pollutants.
- Actions to reduce GHG emissions can bring important reductions in US air pollutants emitted from the same sources, with local and near-term benefits to health that exceed costs in the large majority of studies (West et al. 2023).
- To address these concerns, the DOE CWG would need to:

    - Properly define what they mean by a ‘long delay’, given that CO2 emissions have an immediate impact on radiative forcing and thus temperature, and their cessation leads to immediate decreases in temperature and concentrations
    - Explain that the majority of people in the U.S., therefore, will feel the immediate impact of CO2 emissions.
    - And, thus, describe the local impacts that would occur to people in the U.S., such as changes in temperature, extremes, and more.
    - Re-evaluate their statement that conventional air pollution is local, and situate CO2 better within the span of various pollutants and their health impacts.

Response to 12.2:

Comment: Technical, Editorial

Eighth, the DOE CWG argues that because the climate impacts of regulations on just one economic sector are minimal, then the climate impacts of “the most aggressive regulatory actions on GHG emissions” (p. 130) are small.

- Using the case of U.S. motor vehicle emissions, the DOE CWG justifies the limited impacts of emissions regulations in general by limiting their impact analysis to a single sector. This is a deceptive rhetorical tool referred to as the fallacy of inconsequentialism (Sandler 2010). Such statements have been used in the context of climate change to justify continued emissions by any individual given the globality of anthropogenic emissions (Sandberg 2011).
- To address this problem, the DOE CWG needs to:
    - Re-assess the United States contributions to emissions (cumulatively) and thus assess climate change through a lens that is not driven by inconsequentialism but rather by the accumulation of collective action across sectors and activities.

Comment: Technical, Editorial

Ninth, the DOE CWG asserts that the emissions reductions from regulations on U.S. motor vehicles are not large because the 1.05 billion tonnes of CO2 from U.S. cars and light trucks “amount for only 3.0 percent of global energy-related CO2 emissions” (p. 130).

- This is a false equivalence, as the appropriate comparison would be to global vehicular emissions rather than all worldwide energy-related emissions. Estimates from Lekaki et al. (Lekaki et al. 2024) using the EDGAR database show that global road transport emissions in 2020 (the most recent year of reliable available data for global

road-transport emissions) were 6.204 billion tonnes of CO2. Thus emissions from U.S. cars and light trucks amount to 16.9 percent of global road-transport CO2 emissions, which is nearly six times larger than the 3.0 percent of total emissions stated in the DOE CWG, and by itself a considerable share of emissions.

- Furthermore, temperature is related to cumulative CO2 emissions. Thus, comparing one individual year of U.S. motor vehicles is an irrelevant argument to the impact that it has on climate. The U.S. would not stop motor vehicle emissions for one year only from a regulation; this regulation would have a time-compounding impact on emissions over multiple years.
- The DOE CWG claims that reductions in U.S. vehicle emissions would be unmeasurable. However, even if we cannot directly measure the temperature response of a particular policy, that doesn't mean we cannot estimate its impact. Given our high-confidence understanding of the physics of the climate system, fine tuned over the last 200 years, we can make good estimates of the impacts of any CO2-reduction policy. While there is uncertainty in any calculation like this, the uncertainty is bounded and the bounds do not include zero.
- To address these issues, the DOE CWG would need to:
    - Re-assess the impact of United States emissions in accordance with their cumulative future impacts, particularly due to policies that will have a longer than one year timescale

Response to 12.3:

Tenth, the concluding paragraph of DOE CWG Chapter 12 (p.130) creates a false impression of a static world in which other nations are not themselves seeking to reduce GHG emissions. The conclusion fails to consider the role that US action or inaction influences international ambition, since the US is the second most important emitter currently, and most important historically. It further fails to accurately account for the historical contribution made by US CO2 emissions to cumulative anthropogenic CO2 concentration increases, and fails to accurately assess the contribution made by US greenhouse gas emissions to anthropogenic radiative forcing to date. Finally, it fails to accurately model how different future GHG policy trajectories within the U.S. might influence the global climate in the future, and how different global GHG policy outcomes might influence the domestic climate within the US.

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Public Comment on the DOE Climate Report: Climate Change Impacts to Wildlife and Biodiversity

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Comment Type: Technical, Entire DOE report: pp. 1-139.

Summary: The DOE's July 2025 review of climate impacts of greenhouse gas emissions provides no substantive discussion of climate change impacts on U.S. wildlife and biodiversity, mentioning "biodiversity" only once (p. 80) and never mentioning "wildlife". This omission ignores a large and growing body of evidence showing that greenhouse gas-driven climate change is already causing species declines, range shifts, phenological mismatches, mass die-offs, and heightened extinction risks in both terrestrial and marine environments.

These systems provide important ecosystem services to humanity. Changes in wildlife and biodiversity can therefore have important negative impacts on humanity. By omitting any discussion of this topic, the report implicitly assigns zero cost to such changes, a conclusion not backed by the peer-reviewed literature. Biodiversity loss is not only an ecological crisis but a direct threat to people, undermining food security, fisheries, and increasing disease risks.

To be scientifically credible, the DOE must add a dedicated synthesis of observed and projected climate impacts on biodiversity, drawing on the best available peer-reviewed science. This synthesis should quantify current and future risks, explain ecological and cascading economic consequences, and address terrestrial, freshwater, and marine environments alike. DOE should also include an economic analysis of biodiversity loss, highlighting costs to agriculture, fisheries, tourism, cultural heritage, and public health.

Introduction: The DOE's *A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate* (July 2025) gives extremely limited attention to climate impacts on wildlife and biodiversity. The report contains no synthesis of climate-driven threats to either wildlife or biodiversity, with the exception of a discussion of effects of ocean acidification on corals (pp. 3, 7, 8, 124). The term "wildlife" does not appear, and "biodiversity" appears only once in passing

(p. 70), in the context of a broader discussion on vegetation changes, without any substantive treatment of the subject. Given the overwhelming evidence that climate change is already harming plant and animal populations in the United States and globally, this omission represents a major gap in the DOE report.

Peer-reviewed studies consistently show that climate change is altering species' ranges, phenology (including migration and flowering timing), physiology, reproductive success, and population viability (Pecl et al. 2017; IPBES 2019; McElwee et al. 2023). More than 3,500 assessed wild animal species globally are threatened with extinction by climate change, with terrestrial and aquatic invertebrates especially at risk (Ripple et al., 2025). Assessing the state of evidence for the US, the Fifth National Climate Assessment Chapter 8 on Ecosystems, Biodiversity and Ecosystem Services notes widespread risks to species and ecosystems, many of which have high economic and cultural values (McElwee et al., 2023).

DOE Report Omissions: Here we provide some examples of topics that should have been but were not included in the DOE report. Rising frequencies of unusually hot days have been linked to heightened local extinction rates, reduced colonization, lower site occupancy, and declines in bumble bee (*Bombus* spp.) species in North America and Europe (Soroye et al. 2020). These pollinator declines threaten food production and ecosystem stability. Without reduction of greenhouse gas emissions, nearly half of insect species and about a quarter of vertebrate species globally could lose most of their geographic ranges by 2100 (Warren et al., 2018). These losses would have severe consequences for ecological relationships, food webs, and human well-being.

Another clear example of climate impacts on food webs are changes in phenology, including documented evidence in the US of changes in timing of seasonal events such as leaf-out and flowering, egg hatching, and migration, among others (McElwee et al., 2023; Inouye, 2022); these changes can lead to timing mismatches where interacting species (such as plants and pollinators) change at different rates (Kharouba et al., 2018). Phenological changes can cause significant economic losses, including increased susceptibility to spring frosts and tourism impacts (Inouye, 2022).

In the U.S. marine environment, climate-driven extreme events have already caused large-scale population die-offs. Since 2018, over 10 billion snow crabs (*Chionoecetes opilio*) in the Bering Sea off Alaska have disappeared, apparently from starvation linked to a marine heatwave (Szuwalski et al., 2023). Similarly, the 2015–2016 North Pacific marine heatwave led to the deaths of an estimated 4 million common murres (*Uria aalge*) (Renner et al., 2024) and a ~71% decline in Pacific cod (*Gadus macrocephalus*) abundance, caused by increased metabolic demand and reduced prey availability (Barbeaux et al. 2020). Impacts on U.S. coral reefs are being threatened by ocean acidification (Section 2.2.2), oxygen depletion, marine heat waves and hurricanes that have caused loss of biodiversity and the habitats themselves (Walton et al., 2018; Jones and Gilliam, 2024). The value of US coral reefs in reducing vulnerability to floods and storm damage is valued at more than $1.8 billion annually (Reguero et al., 2021).

Greenhouse gas-driven climate change is also a major driver of vulnerability to disease among species, including expanded ranges for disease vectors, longer transmission seasons, and altered populations of disease-carrying organisms (Pfenning-Butterworth et al., 2024). Diseases often thrive where other stressors are present (e.g., temperature variation and extreme events, changes in habitats, etc.) (Carlson et al., 2022). Disease and pest outbreaks driven by climate changes and extremes can cause significant economic losses. For example, harmful algal blooms that produce neurotoxins can cause severe impacts on wildlife, human health,

recreational and commercial fisheries, and regional economies: a massive algal bloom of *Pseudo-nitzschia* along the West Coast in spring 2015 linked to a marine heat wave led to delays and closures of the Dungeness crab, rock crab and other fisheries alongside deaths of hundreds of marine mammals and birds, costing tens of millions of dollars in lost revenue (Moore et al. 2019; Trainer et al. 2020).

Recommendations

The DOE should address how climate change is disrupting wildlife and biodiversity through shifting ranges and migration patterns, mismatches between species and their food sources, reduced reproduction and survival, physiological stress from heat extremes, and population declines or extinctions. These changes, including the loss of sea ice-dependent, alpine, and Arctic species and the spread of invasive species, are altering predator-prey relationships, increasing disease risks, and destabilizing ecosystems that underpin human well-being. The resulting economic impacts include reduced pollination and fisheries yields, damage to aquaculture, declines in ecotourism, costs from invasive species and harmful algal blooms, and cultural losses tied to species extinctions. Because ecosystem services are interconnected, such impacts can cascade, amplifying social and economic risks. It is critical to recognize that disruptions to biodiversity directly imperil people. Human well-being is intertwined with the stability of natural systems: from safe food and clean water to disease regulation, mental health, and cultural identity. By neglecting biodiversity impacts, the DOE report misses one of the clearest pathways by which greenhouse gas emissions endanger human health and prosperity.

Without such treatments, the DOE report fails to capture one of the most consequential and clear dimensions of climate change in the US.

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Historical Trends and Climate Change

John Nielsen-Gammon

Part II of the report is devoted primarily to analyses of historical trends.

Analysis of historical trends is one of the three primary lines of evidence for determining the impacts of anthropogenic emissions on the climate system, the other two being simulations of the climate system and basic physical understanding. While climate models have their shortcomings (I call them very accurate simulations of imaginary planets similar to our own), one can conduct experiments with them to isolate various causal factors and their impacts on phenomena large enough to be simulated. Physical understanding is sometimes the most powerful tool, but it is limited by the complexity of the climate system.

The challenge with interpretation of historical trends is that they are affected by many things, such as local changes in land cover, agricultural and forestry practices, instrumentation, ocean temperature patterns, and the randomness of the weather. The focus on historical trends is framed on page 46 as whether there are “statistically significant long-term trends over the available historical record” and on page 93 as “whether a signal has been detected in historical observations”.

Why would the presence of a statistically significant trend be important? It could be a sign of a process in the climate system in need of explanation (rather than just some random variation). More often, though, in the context of scientific knowledge of the climate system, a statistically significant trend can serve as evidence or quantification of a signal predicted by other lines of evidence.

The many factors affecting historical trends means that it’s rarely so simple. It’s often necessary to account for the effects of all important factors before the magnitude of the climate change factor can be isolated. Alternatively, one can look for key temporal or spatial patterns, or fingerprints, that would only be expected if climate change were the causal mechanism.

What if there’s not a statistically significant trend? A moment’s thought should be enough to show that the lack of a statistically significant trend, by itself, has very little information content one way or the other. Indeed, to say anything useful about the effect of climate change, it’s necessary to account for the other factors and their effects too. If those effects are small enough, it may be possible to put useful upper and lower bounds on the effect of climate change.

If one is interested in the climate change signal, one should focus on as large a geographical area and as long a time series as possible, because the effects of ocean temperature patterns, the randomness of the weather, and local changes in land cover, agricultural and forestry practices become averaged out to varying extents, improving the climate change signal to noise ratio. Once the climate change signal has been detected at the large scale, the question becomes not whether a signal is detectable at every single location (it won't be, because of weather randomness etc.), but how large the local signal ought to be, given the existence of the large-scale signal. Parts of the report do focus on global annual metrics, for which the magnitude of other confounding factors is expected to be small. But others do not.

For example, Figure 6.3.3 showed that record warm season maximum temperatures peaked in the United States in the 1930s. This is clearly not a climate change signal, as no known climate forcings or global climate signals maximized in the 1930s. So how large is the climate change signal? It's very difficult to say. If you're willing to assert that whatever was happening in the early part of the 20th century is no longer happening, then you maybe could look at the trend since 1975 or so and find an upward trend in record highs and an upward (negative) trend in record lows. But really, you need to figure out why extreme temperatures were so large in the 1930s (and 1950s) and subtract out that effect before you could say what the climate change trend is.

The report doesn't do that. That's fine if the objective is simply to report on the historical trend. But the report is titled "A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate", and simply reporting the historical trend and its significance (or lack thereof) in isolation says nothing about the impacts of greenhouse gas emissions.

Given that the trend in extreme heat is dominated (at least prior to 1970) by effects other than climate change, estimates of the magnitude of the effects of climate change must come via other means. Those include climate model simulations, which can be post-calibrated to the historical mean temperature trend, and basic physical understanding of how extreme temperatures ought to be affected by changes in average temperatures.

Figure 5.9 of the DOE CWG report examines a trend that's not the opposite sign of climate models but still substantially different: summertime maximum temperatures in the Corn Belt. This is again a single season, but now also a much smaller area than the whole United States. Thus, natural variability is expected to play a greater role. Since the Corn Belt has been selected for this report precisely because it has the most anomalous regional trend, the a priori expectation would be that natural variability has had a negative effect on maximum temperature trends. Eischeid et al. (2023 JClim

https://doi.org/10.1175/JCLI-D-22-0716.1) confirms this, finding from large ensembles that half or more of the discrepancy between a lower-sensitivity model and observations in recent years is due to unforced precipitation variability. Another potential contributing factor is agricultural intensification in the region, as noted by Eischeid et al. As with the record warm season maximum temperatures, climate models can be and are useful for estimating the trend magnitude that's attributable to climate change as separate from natural variability and agricultural intensification; the difference between models and observations tells us something about the other processes that affect temperature locally and does not automatically mean that there's something wrong with the models.

Section 6.4 examines extreme precipitation trends in three subregions of the United States, collectively representing perhaps 35% of its land area. The choice of small areas (and, in the case of the southeast US, a sparse sampling network) lead to a correspondingly large contribution to natural variability. Nevertheless, there is a notable trend in the number of extreme rainfall events in both the southeast US and the northeast US. In the southeast, the trend in number of events per decade is +0.50, 95% confidence interval -0.18 to +1.17. (All analyses here use ordinary linear regression.) Notice that, because of natural variability, a trend corresponding to a 20% increase in the frequency of extreme rainfall over the past century is nonetheless not statistically significant. In the northeast, the trend in number of events per decade is +1.70, 95% confidence interval +0.66 to +2.74, thus is statistically significant.

To see if there's an overall trend, I combined the counts in the three areas, dividing the northeast counts by two because they represent a smaller geographic area. Perhaps the weight should be even smaller, but the larger number of Pacific Coast stations arguably unduly weights that area higher than the southeast. (Arguably the regression should be negative binomial rather than linear, and with respect to global mean surface temperatures rather than time.) Be that as it may, the aggregate trend is +1.85 events per decade, 95% confidence interval +0.03 to +3.66. This trend, which is marginally statistically significant, corresponds to about a 30% increase in the frequency of extreme rainfall over the past century.

In summary, the smaller the sample size (number of stations, geographical area, time of year, period of record), the more that natural variability and local effects will tend to overwhelm the climate trend. The trend analyses discussed above failed to find a trend (or agreement with climate models) in part because they focused on relatively small areas. The lack of statistical significance does not imply the absence of an underlying climate trend, and more powerful analyses covering broader areas or including more data are needed to determine whether the results from the smaller areas are representative.

In all of the cases discussed above, the report should make clear that the analyses cover limited areas and periods for which the effect of natural variability and local processes are expected to be relatively high, and that more robust analyses for estimating the effect of climate change would cover larger areas and periods.

Comments on the scientific information quality of the DOE CWG report

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These comments address issues related to the DOE CWG report's development process and are in the "other" category. They apply to the entire DOE CWG report.

Summary: The DOE CWG report meets the criteria set by EPA as a Highly Influential Scientific Assessment (HISA) and is subject to EPA criteria because of its preeminent role in an EPA rulemaking. The DOE CWG report fails to meet EPA's standards for information quality required for HISAs used in major agency decisions or actions. The report was not subjected to independent and transparent peer review. The report was not made available for public comment. The report demonstrates a lack of objectivity in its development and its content. Neither DOE nor EPA have provided any compelling reason for failing to follow information quality practices set forth by OMB, DOE, or EPA.

The DOE CWG report meets EPA's criteria as a Highly Influential Scientific Assessment

The DOE CWG report meets EPA's criteria that a Highly Influential Scientific Assessment (HISA): "(i) could have a potential impact of more than $500 million in any year, or (ii) is novel, controversial, or precedent-setting or has significant interagency interest" (USEPA, 2015) The proposed rule meets both these criteria.

The DOE CWG report is subject to EPA's information quality guidelines

EPA's information quality guidelines apply when it "distributes information prepared or submitted by an outside party in a manner that reasonably suggests that EPA endorses or agrees with it" (USEPA 2002). The preamble to the proposed rule states that "the Administrator received and evaluated" a draft of the DOE CWG report prior to the report's release (USEPA 2025). The DOE CWG report was frequently and prominently cited, demonstrating that EPA does in fact endorse or agree with the report. The DOE CWG report is therefore subject to EPA's information quality guidelines.

The DOE CWG report fails to meet EPA's information quality standards for HISAs

A core component of EPA's Quality System, which is designed to ensure EPA achieves its information quality standards, is rigorous peer review. EPA's Peer Review Handbook states that products deemed to be HISAs "are expected to undergo rigorous external

peer review with opportunities for public participation." The Handbook further states that, "For ISI and HISAs intended to support the most important decisions, or for work products that have special importance in their own right, the recommended approach is an internal review followed by an external peer review" (USEPA 2015).

A critical characteristic of external peer reviewers is independence, that is, reviewers "are generally not employed by the agency or office producing the document" (OMB 2005). The CWG Report was reviewed only by DOE employees. Further, the review of the CWG Report failed to follow good peer review practices as described by the Office of Management and Budget's guidelines to ensure transparency, including publication of reviewers' names, charge questions to the reviewers, reviewers' comments, and DOE's response to review comments (OMB2005). Contrary to the EPA peer review guidelines, the review did not provide opportunities for public participation.

The DOE CWG Report demonstrates a lack of objectivity

Per OMB, objectivity "includes whether disseminated information is being presented in an accurate, clear, complete, and unbiased manner" The CWG Report not only fails to demonstrate processes to maximize objectivity, its development and content makes plain its intent to present a biased and predetermined view of climate science. As one climate scientist explained, "Their goal is not to weigh the evidence fairly but to build the strongest possible case for [carbon dioxide's] innocence. This is a fundamental departure from the norms of science." (Dessler, Science)

Rather than demonstrating a sincere effort to produce a scientifically objective report, the development process followed by the DOE CWG report instead creates a strong impression of bias toward a specific desired policy outcome. The slate of authors appears to have been selected specifically because of their long-standing critiques of the scientific consensus regarding climate change. The number of authors is small, which greatly reduces the opportunity to hear and incorporate a diverse range of perspectives. DOE did not subject the draft report to independent peer review before presenting the report as suitable for use in a major policy action, with the knowledge that the policy action was contrary to the scientific consensus. The report largely ignores the vast body of climate science except to critique it. Each of these characteristics undermines the perception of objectivity.

Objectivity is impossible to prove, and likely impossible to achieve. Perhaps the report is objective and chooses to only emphasize topics that need to be evaluated further. There is value in such an approach, but it must be conducted in such a manner that builds, rather than undermines, trust. An objective approach would review the full body of evidence on a particular topic, even when emphasizing the need for further evaluation.

When combined with the serious shortcomings of the scientific content, however, the lack of robust processes designed to build confidence in the report's objectivity creates a perception of bias.

Finally, a lack of objectivity does not imply malintent by the authors. Bias can arise well apart from bad faith actions. The processes and procedures described in the OMB and EPA guidelines are designed to substantially increase the objectivity of a research document as well as the perception of objectivity. But it is unrealistic to think that anyone involved in the development of the DOE CWG report could be unaware that the release of the report would stir substantial controversy. For that reason alone, the authors and the Secretary should have insisted on an open and rigorous review process to demonstrate the strength and scientific validity of their conclusions. That they apparently did not insist on such a review makes it more difficult to discount an intention to present conclusions that lack the objectivity essential to a document playing such a crucial role in setting such a consequential policy.

No compelling reason has been provided to waive provisions for peer review

EPA's Peer Review Handbook states that the "Administrator may waive or defer the peer review provisions of the OMB Peer Review Bulletin for ISI [influential scientific information] (including HISAs) if there is a compelling rationale for the waiver or deferral" (EPA 2015). The Handbook also notes that "The use of waivers is expected to be limited to unusual and compelling situations not otherwise covered by the exemptions, such as situations in which unavoidable legal deadlines prevent full implementation of the OMB Peer Review Bulletin's peer review provisions (EPA 2015). However, there is no indication that such waiver or deferral has been granted and there are no "unavoidable legal deadlines" or similar situations that would meet the criteria of "unusual and compelling situations."

Resolution

Given the major issues with the development of the DOE CWG report that have called into question the quality of the scientific information disseminated in the report, DOE has two options to resolve the issues: (1) retract the report; or (2) use the review announced by the National Academies of Sciences, Engineering, and Medicine as a peer review, revise the draft report in response to that review and the relevant comments received from the public, and publish the list of comments and authors' responses on a point-by-point basis.

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DOE CWG Draft Report Citations Review

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A review of the in-text citations and references list in the DOE CWG Draft Report reveals pervasive problems with misrepresentative, inaccurate, and missing citations, citations of non-peer-reviewed pre-prints and blog posts to justify scientific claims, and a high proportion of self-citations by the DOE CWG Draft Report authors.

Misrepresentative, inaccurate, and missing citations:The DOE CWG Draft Report contains 367 in-text citations (non-inclusive of citations of the IPCC and NCA reports) referring to a total of 284 unique references. Of those 367 in-text citations and the references to which they index, at least 22.1% (81 in-text citations or their indexed references) are inaccurate or misrepresentative. An additional 6 in-text citations are entirely missing full references.

Examples of misrepresentation of content from in-text citations include:

- Chapter 1, page 2 of the DOE CWG Draft Report misreports key numbers from a source. The DOE CWG claims that “Allen et al. (2015) reported evidence of diminished performance on some cognitive tasks among workers in office cubicles when exposed to CO2 levels above 1,000-1,500 ppm.” This in-text citation and associated summary of scientific findings refers to the listed reference for “Allen, J., Macnaughton, P., Satish, U., et al. (2015). Associations of cognitive function scores with carbon dioxide, ventilation, and volatile organic compound exposures in office workers: A controlled exposure study of green and conventional office environments. *Environmental Health Perspectives*. 124. https://doi.org/10.1289/ehp.1510037.” In fact, Allen et al. (2015) studied experimental conditions with targets of 550ppm, 945ppm, and 1400ppm, not a range of “1,000-1,500 ppm,” as the DOE CWG Draft Report claims. While this error may not change the fundamental meaning of the claims related to human exposure to CO2, it does illustrate how inaccuracies appear not just in the DOE CWG Draft Report citations and reference lists themselves, but also in the descriptions of the studies these citations and references point to.
- Chapter 2, page 3 of the DOE CWG Draft Report contains a similar misrepresentation. The claim that “Haverd et al. (2020) reported a CO2 fertilization rate much larger than model predictions. That is, CO2 fertilization had driven an increase in observed global photosynthesis by 30 percent since 1900, versus 17 percent predicted by plant models” overstates the certainty of Haverd et al.’s findings, which predicted 17+/-4%, not 17%.
- Chapter 8, page 88 of the DOE CWG Draft Report cites sources that do not support the claim they are appended to, either because they do not provide the data the DOE CWG states they or or because their data differ significantly what the claim made by the DOE CWG. This section of the DOE CWG includes the claim that, “When the Great Pacific

Climate Shift is accounted for in climate attribution analyses since 1950, 40 percent or more of the warming in the second half of the 20th century is attributed to natural internal variability (McLean et al., 2009; Tung and Zhou, 2013; Chylek et al., 2016; Scafetta, 2021)." The authors of the response to Chapter 8 point out that three of the four sources in this in-text citation do not support the claim in the sentence they are appended to: "Specifically, McLean et al. (2009) and Scafetta (2021) provide no quantitative estimates of the contribution of internal variability to the warming trend. Moreover, the estimate presented in Chylek et al. (2016) is substantially lower than that of Tung and Zhou (2013). According to Chylek et al. (2016), the contribution of the Atlantic Multidecadal Oscillation (AMO) to the 1970–2005 warming was between 0.13 and 0.20 °C (depending on the AMO index used), compared with a greenhouse gas contribution of 0.49–0.58 °C." [Response to Section 8.3.1]

- In another example of misreporting key details about a source, Chapter 9, page 107 of the DOE CWG Draft Report includes the statement that, "McKitrick (2025) re-examined the Moore et al. database and found that, while it claimed to cover 1,722 studies, only half the entries (N=862) had complete records, so that the sample available for regression analysis was much smaller than both studies indicated." The authors of the response to Chapter 9 correct this statement: "Nowhere in Moore et al. (2017) is it claimed that 1,722 observations were used for the analysis. Moore et al. (2017) report using 1,010 observations, derived from the subset of Challinor et al. (2014) data relevant to the 4 crops studied (rice, wheat, maize and soybeans), where complete data was either present or could be inferred from reported information." [Response to Section 9.3]
- Chapter 9, p. 108 of the DOE CWG Draft Report cites a source which cannot "prove" the claim it is appended to. The DOE CWG claims that "Micronutrient deficiency is already a problem in the developing world and dietary supplements have proven to be an effective low-cost response (Ebi et al. 2021)." The authors of the response to Chapter 9 straightforwardly note that this reference cannot support this claim: "Ebi et al. (2021) discuss dietary supplements but do not conclude they have "proven to be an effective low-cost response" to declining nutrient density in crops." To correct this misrepresentation of the content of Ebi et al. (2021), the DOE CWG Draft Report authors must "remove the statement that dietary supplements have proven to be an effective low-cost response to micronutrient deficiency, or they need to find a source other than Ebi et al. (2021) to support this statement. Ebi et al. (2021) does not make this claim." [Response to Section 9.4]
- Chapter 11, p. 119 of the DOE CWG Draft Report incorrectly states the object of a reference's study. While the DOE CWG claims that, "Greβer et al. (2021) developed a regional economic data set for 1,542 sub-national regions around the world between 2005 and 2015 and found temperature had no effect on growth," in fact, "Gresser, Meierrieks, and Stadelmann (2021) do not study the impact of climate on growth. They study its impact on *output*." [Response to Section 11.1]

Many more examples of misrepresentation of content are included in comments on specific chapters of the DOE CWG Draft Report.

Examples of inaccurate in-text citations and references include references with sufficient inaccuracies as to appear potentially fabricated as well as relatively more minor errors, such as mismatches between dates on in-text citations and entries in the reference lists, incomplete and incorrect author lists, incorrect DOIs (direct object identifiers), and incorrect pages. Specific examples are:

- Chapter 4, page 28 of the DOE CWG Draft Report cites Lee et al. (2024), indexing to the following reference in that chapter's references list: Lee, S., Byrne, M. P., Loikith, P. C., & O'Dell, C. W. (2024). Zonal contrasts of the tropical Pacific climate predicted by a global constraint. *Climate Dynamics*, 62(1–2), 229–246, https://doi.org/10.1007/s00382-023-06741-7. This article, however, does not exist, and the hyperlinked DOI directs to an unrelated 2023 article in the journal *Climate Dynamics* titled "Quantifying the processes of accelerated wintertime Tibetan Plateau warming: outside forcing versus local feedbacks" authored by Mengchu Zhao, Xiu-Qun Yang, and Lingfeng Tao. There *is* a 2024 article titled "Zonal contrasts of the Tropical Pacific Climate Predicted by a Global Constraint," but it was authored by Sukyoung Lee, Peter R. Bannon, Mingyu Park, and Joseph P. Clark and published in Volume 61, pp. 669-678 of the *Asia-Pacific Journal of Atmospheric Sciences,* with the assigned DOI of https://doi.org/10.1007/s13143-024-00373-5. While the DOE CWG Draft Report authors may have been intending to cite this latter article, it is unclear how the author list, journal title and volume, page numbers, and DOI were incorrectly listed in the report's Chapter 4 reference list.
- Chapter 8, page 90 of the DOE CWG Draft Report includes an in-text citation of "Goesslin et al. (2025)," which indexes to the reference "Goessling, Helge F. Thomas Rackow and Thomas Jung (2024) Recent global temperature surge intensified by record-low planetary albedo. *Science* Vol 387, Issue 6729 pp. 68-73 https://www.science.org/doi/abs/10.1126/science.adq7280." *Science's* publication information (available at https://www.science.org/doi/abs/10.1126/science.adq7280) lists 2025 as the correct date.
- Chapter 9, page 105 of the DOE CWG Draft Report includes an in-text citation of "Allen Jr., 2011," which indexes to the reference "Allen Jr, L. K. (2011). Elevated CO2 increases water use efficiency by sustaining photosynthesis of water-limited maize and sorghum. *Journal of Plant physiology*, 168(16), 1909-1918." This article has three additional authors (Vijaya Gopal Kakani, Joseph C. V. Vu, and Kenneth J. Boote), who were included in neither the DOE CWG Draft Report's in-text citation or reference list.
- Chapter 2, page 6 of the DOE CWG Draft Report includes an in-text citation of "Zhang et al. (2024)," which indexes to the reference "Zhang, Y., Liu, Y., Chen, X., et al. (2024). Less than 4% of dryland areas are projected to desertify despite increased aridity under climate change. Nature Communications Earth and Environment, 5. https://www.nature.com/articles/s43247-024-01463-y." All authors in this reference are incorrect; the correct author list is Xinyue Zhang, Jason P. Evans, and Arden L. Burrell.
- The DOE CWG Draft Report reference list for Chapter 4 (pp. 29-30) includes the reference "Lewis, N. (2023). Objectively combining climate sensitivity evidence. Climate Dynamics, 61(9–10), 3155–3163. https://doi.org/10.1007/s00382-022-06398-8." This DOI directs to an entirely different article: Chen, Z., Gan, B., Huang, F. *et al.* The

influence of Pacific-North American teleconnection on the North Pacific SST anomalies in Wintertime under the global warming. *Clim Dyn* **60**, 1481–1494 (2023). https://doi.org/10.1007/s00382-022-06398-8.

- Chapter 2, page 8 of the DOE CWG Draft Report includes an in-text citation of Browman (2016) indexes to "Browman, H. I. (2016). Applying organized scepticism to ocean acidification research. ICES Journal of Marine Science, 73(3), 529.1–536. https://doi.org/10.1093/icesjms/fsw010." The correct page range is 529-536.

The following six in-text mentions of literature do not have references listed in their respective chapter reference lists (six have in-text citations and no reference; one has neither in-text citation nor reference):

1. Chapter 4, p. 28: "About 15 estimates based on historical data appeared in the peer-reviewed literature between 2012 and 2024 yielding ECS best estimates between 1.0°C and 2.5°C, although critics have questioned some of the methods and the data quality." No in-text citations or references are given to source the claim about 15 estimates based on historical data appearing in the peer-review literature.
2. Chapter 4, p. 25: "The 1979 Charney Report for the U.S. National Academy of Sciences (National Research Council1979) proposed that the most likely ECS was 3.0 ± 1.5°C." National Research Council 1979 has no reference listed in the Ch. 4 reference list.
3. Chapter 6, p. 56: "The rise in Tmin has been strongly related to the growing presence of manufactured surfaces around the weather stations over the last 100+ years (the so-called urban heat island effect; Section 3.3, Karl et al. 1988, Runnals and Oke 2006, and Spencer et al. 2025)." Spencer et al. 2025 has no listed reference in the Ch. 6 reference list.
4. Chapter 6, p. 57: "For example, the 5-day average tropospheric grid-point temperature anomaly over the Pacific NW during that event was +10.8 C, the most extreme Northern Hemisphere grid point summer anomaly in the 46 years from over 4 million grid values. In contrast, the global temperature anomaly during that time was virtually zero (+0.03 C, Mass et al. 2024)." Mass et al. 2024 has no listed reference in the Ch. 6 reference list.
5. Chapter 8, p. 87: "Bronniman et al. (2024) focused on the causes of the cooling in the first decade of the 20th century in the Southern Hemisphere. They found that the cooling was related to a La-Niña-like pattern in the Pacific, a cold tropical and subtropical South Atlantic, a cold extratropical South Pacific, and cool southern midlatitude land areas. The Southern Annular Mode was positive, with a strengthened Amundsen–Bellingshausen seas low, although the spread of the data products is considerable." Bronniman et al. (2024) has no listed reference in the Ch. 8 reference list.
6. Chapter 9, p. 104: "Deschênes and Greenstone (2007) looked at agricultural profits instead of land values and reached conclusions similar to Mendelsohn et al. (1994), namely that past climatic variations had relatively little effect on farm profitability and that warming would likely yield small overall benefits for the U.S. agricultural sector." The Ch. 9 reference list includes a reference for the 2012 reply by Deschênes and Greenstone to critiques, but not a reference for the original 2007 paper.

Citations of non-peer-reviewed pre-prints and blog posts to justify scientific claims: Of the DOE CWG Draft Report's 284 unique references, 4 are to Substack posts ("Pielke Jr., R. (2025, May 8). RIP NOAA's billion dollar disasters. The Honest Broker [Substack]. https://rogerpielkejr.substack.com/p/rip-noaas-billion-dollar-disasters"; "Pielke Jr., Roger (2024) Weather Attribution Alchemy. Substack essay https://rogerpielkejr.substack.com/p/weather-attribution-alchemy") and non-peer-reviewed pre-prints ("Pielke Jr., Roger and Ritchie, Justin (2020) "Systemic Misuse of Scenarios in Climate Research and Assessment" Social Sciences Research Network April 2020, available at: https://ssrn.com/abstract=3581777"; "McKitrick, Ross R (2025) Consistent Climate Fingerprinting. Climate Dynamics forthcoming preprint available at www.rossmckitrick.com"). These are cited 7 times in the DOE CWG Draft Report to justify scientific claims.

Self-citations by the DOE CWG Draft Report authors: Self-citations (citations to work on which they are a sole or co-author) by the DOE CWG Draft Report authors make up a notably high proportion of both in-text citations and unique references. 10.9% (40) of the report's in-text citations are self-citations; these index to 8.8% (25) of the report's 284 unique references. In chapters 3 and 5, more than a quarter of in-text citations are self-citations (Table 1).

Chapter	In-Text Citations	Self-Citations	% Self-Citations
1	3	0	0.00%
2	32	0	0.00%
3	37	10	27.03%
4	22	2	9.09%
5	17	5	29.41%
6	52	6	11.54%
7	9	0	0.00%
8	80	9	11.25%
9	26	1	3.85%
10	29	0	0.00%
11	56	7	12.50%
12	4	0	0.00%
Total	*367*	*40*	*10.90%*

Table 1. Total count of in-text citations, total count of self-citations, and percentage of self-citations by chapter.

Median rates of self-citation vary across disciplines, so this rate cannot be interpreted as universally high. However, this rate is notably high in relation to the self-citation rates by authors in IPCC reports. For the IPCC Working Group 1 contribution to AR6, for example, self-referencing rates for each chapter and their respective authors ranged from 1.2% to 5.1% with a mean of 2.4% (Chavelli & Connors, 2023). This relative proportion suggests an

inappropriate overreliance on their own work by the DOE CWG Draft Report authors and a concomitant lack of inclusion of report by the many other experts on the topics covered in the report.

References

Chavelli, F., & Connors, S. (2023). Analysis of the WGI contribution to the Sixth Assessment Report: Review of the WGI AR6 references. Zenodo. https://doi.org/10.5281/zenodo.7615825

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